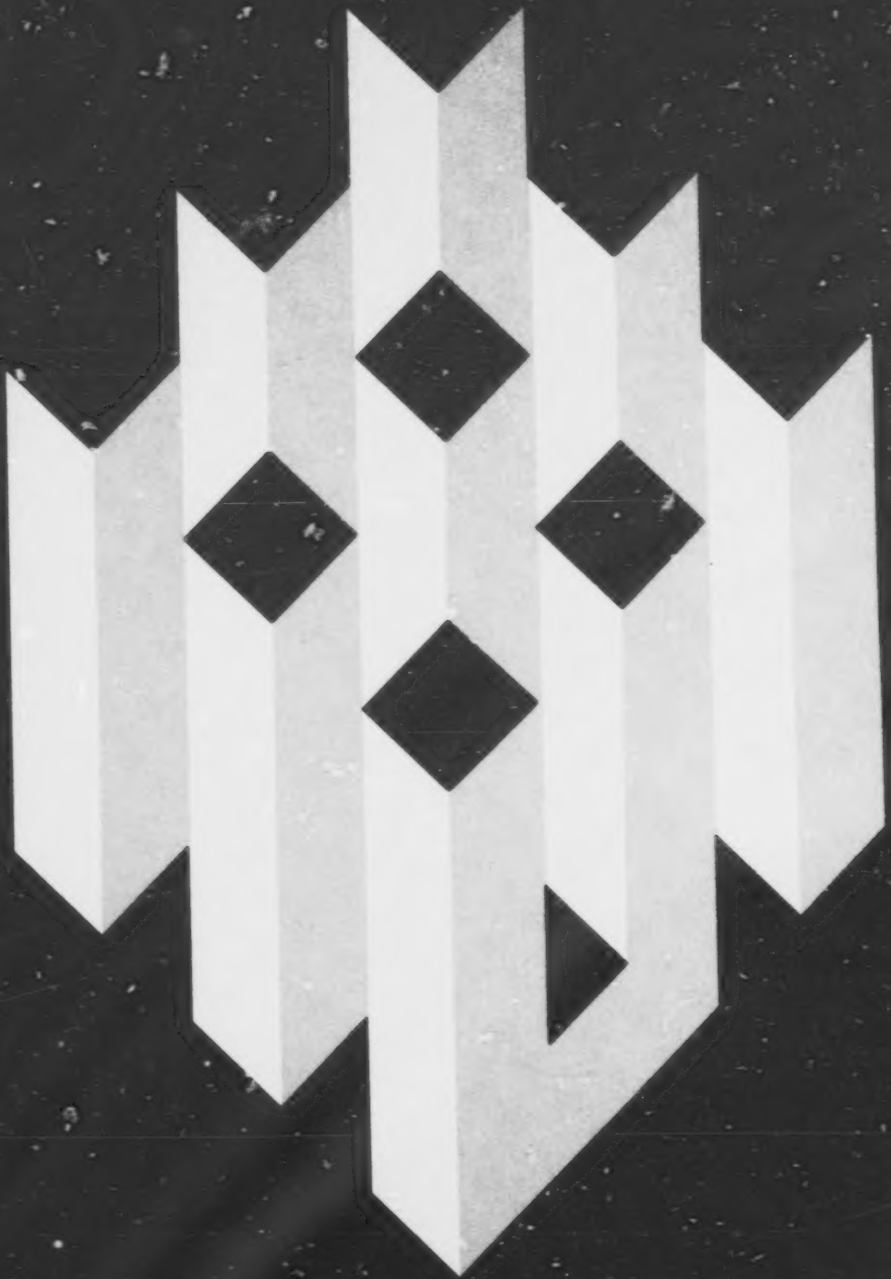
89-3

New Jersey Unemployment Insurance Reemployment Demonstration Project

Unemployment Insurance
Occasional Paper 89-3

U.S. Department of Labor
Employment and Training Administration



L 37.20:

READEX FICHE best copy available

Material contained in this publication is in the public domain and may be reproduced, fully or partially, without permission of the Federal Government. Source credit is requested but not required. Permission is required only to reproduce any copyrighted material contained herein.

New Jersey Unemployment Insurance Reemployment Demonstration Project

Unemployment Insurance
Occasional Paper 89-3

U.S. Department of Labor
Elizabeth Dole, Secretary

Employment and Training Administration
Roberts T. Jones
Assistant Secretary of Labor

Unemployment Insurance Service
Mary Ann Wyrsch, Director

1989

This report was prepared for the Unemployment Insurance Service, U.S. Department of Labor under Cooperative Agreement Number 99-2325-04-055-05 with the New Jersey Department of Labor and Mathematica Policy Research (subcontractor). The authors of this report are Walter Corson, Shari Dunstan, Paul Decker and Anne Gordon of Mathematica Policy Research. Since contractors conducting research and evaluation projects under government sponsorship are encouraged to express their own judgments freely, this report does not necessarily represent the official opinion or policy of the U.S. Department of Labor.

The Unemployment Insurance Occasional Paper Series presents research findings and analyses dealing with unemployment insurance issues. Papers are prepared by research contractors, staff members of the unemployment insurance system, or individual researchers. Manuscripts and comments from interested individuals are welcome. All correspondence should be sent to UI Occasional Papers, Unemployment Insurance Service, Frances Perkins Building, Room S-4519, 200 Constitution Ave., N.W., Washington, D.C. 20210.

Contract No.: S 86042
MPR Reference No.: 7676

THE NEW JERSEY UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT

FINAL EVALUATION REPORT

April 1989

Authors:

Walter Corson
Paul T. Decker
Shari Miller Dunstan
Anne R. Gordon

with:

Patricia Anderson
John Homrighausen

Prepared for:

N.J Department of Labor
Income Security
John Fitch Plaza
Trenton, N.J. 08625

U.S. Department of Labor
Employment and Training
Administration
Unemployment Insurance Service
200 Constitution Avenue, N.W.
Washington, D.C. 20010

Prepared by:

Mathematica Policy Research, Inc.
P.O. Box 2393
Princeton, N.J. 08543-2393

Principal Investigators:
Walter Corson
Stuart Kerachsky

This report was prepared for the New Jersey Department of Labor and the Employment and Training Administration, U.S. Department of Labor under New Jersey contract number S86042 with Mathematica Policy Research, Inc. Since Contractors conducting research and evaluation projects under government sponsorship are encouraged to express their own judgments freely, this report does not necessarily represent the official opinion or policy of the N.J. Department of Labor or the U.S. Department of Labor.

ACKNOWLEDGMENTS

Many individuals deserve recognition for their important contributions to the New Jersey Unemployment Insurance Reemployment Demonstration Project (NJUIRDP) and to the evaluation. At the N.J. Department of Labor, I wish to express special thanks to Frederick Kniesler, who provided overall guidance the throughout the project, and to Nancy Snyder, the Project Manager, and Roger Emig, the Project Supervisor, each of whom helped make the project run smoothly.

Many other N.J. Department of Labor staff members also deserve thanks for their contributions to the evaluation. These individuals shared their observations about the implementation of the demonstration, and they both answered questions and collected and provided special data when requested. In particular, John Kinnard and Janis Geary provided substantial assistance on Unemployment Insurance (UI) operational issues, as did Ronald Pugh and Thomas D'Elia on Job Training Partnership Act (JTPA) issues and Jack Fullmer on Employment Service (ES) issues. Philip LaManna coordinated the collection of administrative data used in the analysis. Vivien Shapiro and her research staff provided useful comments on the draft reports and answered numerous questions during the analytical phase of the project. Throughout the life of the demonstration, the ten local offices were visited many times, and I greatly appreciate the time taken by the local UI, ES, and JTPA staff members to answer our questions. The help received from these individuals and the insights they provided on program operations were extremely valuable, particularly to the implementation and process report.

U.S. Department of Labor staff also contributed substantially to the project and the evaluation. Stephen Wandner, who initiated the project, and Wayne Zajac, the project officer, deserve special thanks. Wayne Gordon shared with us his observations about site operations, and Norm Harvey and Jon Messenger played important roles in the design and in the monitoring process. In addition, Joseph Hight, Douglas Holl, John Robinson, Robert Schaerfl, Ray Uhalde, and Mary Ann Wyrsch provided useful comments on the draft reports. The help and assistance of these individuals is greatly appreciated.

A number of individuals at Mathematica Policy Research provided assistance in the analysis and report production process. In particular, I am greatly indebted to Carl Nelson, who performed many of the site visits and whose observations form the basis of much of the process and implementation report. Were it not for Carl's untimely death, he would have been an invaluable co-author of that report. In addition, John Homrighausen ably directed the survey and wrote an appendix on survey methods and results (a summary is included in this volume). John Crawford and Patricia Anderson developed the data files used in the analysis and performed the analysis programming for, respectively, the process and impact analyses. Patricia was also responsible for the non-response analysis presented in the appendix to this report. Eric Horlbeck also provided valuable programming assistance. Shari Dunstan performed the cost analysis and contributed to the writing of the implementation and process report. Paul Decker and Anne Gordon each made valuable contributions to the analysis and presentation of results for the impact and benefit-cost report. Paul was responsible primarily for the earnings and employment impacts analyses, and Anne performed the benefit-cost analysis. Stuart Kerachsky, the co-principal investigator, carefully reviewed and commented on the analysis and on drafts of the reports, and made many important suggestions that are contained in each report. Finally, Thomas Good provided editorial support and Donna Adubato cheerfully oversaw production of the report. Each of these individuals deserves acknowledgment and my thanks.

Walter Corson

PREFACE

This final evaluation report for the New Jersey Unemployment Insurance Reemployment Demonstration Project consists of three major parts: (1) a short, policy-oriented summary, (2) an implementation and process report, and (3) an impact and benefit-cost report. These three reports are published together here, but they were prepared as separate, stand-alone documents intended for different audiences. For that reason, there is some duplication among the reports, particularly in the description of the project design. Readers should bear this in mind if they wish to examine both the process and implementation and impact and benefit-cost reports.

CONTENTS

EXECUTIVE SUMMARY ix

PART 1 SUMMARY REPORT
by Walter Corson 1

PART 2 IMPLEMENTATION AND PROCESS REPORT
by Walter Corson and Shari Miller Dunstan 21

CONTENTS 23
I INTRODUCTION 29
II THE LOCAL ENVIRONMENTS 37
III IDENTIFYING AND NOTIFYING THE ELIGIBLE POPULATION 47
IV THE INITIAL SET OF COMMON SERVICES 69
V PERIODIC JOB SEARCH ASSISTANCE 89
VI TRAINING AND RELOCATION 103
VII THE REEMPLOYMENT BONUS 121
VIII MONITORING COMPLIANCE 131
IX ORGANIZATION AND STAFFING 145
X COSTS OF THE INTERVENTION 155
XI REPLICABILITY AND SUMMARY 171
REFERENCES 175

PART 3 IMPACT AND COST BENEFIT REPORT
by Walter Corson, Paul T. Decker and Anne R. Gordon 177

CONTENTS 179
I INTRODUCTION 187
II THE DESIGN OF THE NJUIRDP 195
III THE ELIGIBLE POPULATION 215
IV SERVICE RECEIPT 227
V IMPACTS ON UNEMPLOYMENT INSURANCE 253
VI IMPACTS ON EMPLOYMENT AND EARNINGS 279
VII BENEFIT-COST ANALYSIS 317

CONTENTS (continued)

VIII IMPLICATIONS OF THE FINDINGS FROM THE DEMONSTRATION EVALUATION 341

REFERENCES 345

APPENDIX A: STATUS SOURCES 347

APPENDIX B: SURVEY RESULTS AND NON-REPONSE ANALYSIS . . 351

APPENDIX C: COMPARISON OF INTERVIEW AND WAGE RECORDS DATA 363

APPENDIX D: SUPPLEMENTARY TABLES 371

APPENDIX E: BENEFIT-COST ANALYSIS METHODOLOGY AND ASSUMPTIONS 413

THE NEW JERSEY UI REEMPLOYMENT DEMONSTRATION PROJECT
FINAL EVALUATION REPORT
EXECUTIVE SUMMARY

The purpose of the New Jersey Unemployment Insurance Reemployment Demonstration Project (NJUIRDP) was to examine whether the Unemployment Insurance system could be used to identify displaced workers early in their unemployment spells and to provide them with alternative, early intervention services to accelerate their return to work. Three packages of services, or treatments, were tested in the demonstration: (1) job-search assistance only, (2) job-search assistance combined with training or relocation assistance, and (3) job-search assistance combined with a cash bonus for early reemployment. A key component of the demonstration was that eligible claimants were identified and services were provided through the coordinated efforts of the Unemployment Insurance (UI), Employment Service (ES), and Job Training Partnership Act (JTPA) systems. Another key component was that claimants were required by UI to report for services; failure to report could have led to the denial of benefits.

The demonstration was initiated by the U.S. Department of Labor through a cooperative agreement with the N.J. Department of Labor. It began operations in July 1986, and, by the end of sample selection in June 1987, 8,675 UI claimants were offered one of the three service packages in the ten local offices included in the demonstration. Services to eligible claimants were continued into fall 1987 to ensure that all eligibles were able to receive, if desired, the full set of demonstration services. Another 2,385 claimants, who received existing services, were selected to provide a control group for comparative purposes for the evaluation. Assignment to this control group and to the three treatments was random. During the demonstration period, the New Jersey economy was experiencing worker displacement, generated by a long-term secular decline in manufacturing, while substantial growth was occurring in other sectors. Overall, the state economy was quite strong, and the unemployment rate during the period was low (5 percent).

The evaluation of the demonstration consists of three components: (1) a short policy-oriented summary, (2) an implementation and process analysis, and (3) an impact and benefit-cost analysis. In general, the evaluation found that the treatments were implemented as designed. That is, eligible claimants were identified, offered services, and provided services early in their unemployment spell. Moreover, each of the treatments did lead to reductions in the lengths of unemployment spells and to concomitant increases in earnings and reductions in UI benefits received. All three treatments offered net benefits to society as a whole and to claimants, when compared to existing services. These findings can be summarized further as follows.

Eligibility Determination

The demonstration used the UI system to apply eligibility screens in an attempt to target demonstration services toward claimants who were likely to be displaced and who were likely to experience difficulty in becoming reemployed. Based on these requirements, about one-quarter of the claimants who received a first payment were eligible for demonstration services. The most important eligibility screen was the tenure requirement, which excluded individuals who had not worked for their pre-UI employer for at least three years. Other important requirements excluded individuals younger than age 25 and individuals with a definite recall date. The net result of applying the eligibility requirements was an eligible population that contained a substantial proportion of individuals whose age, industry of employment, and other characteristics are usually associated with the displaced worker population and with difficulties in becoming reemployed. Moreover, as compared with a sample of individuals who were not eligible for the demonstration, the eligible population experienced, on average, considerably longer periods of UI collection and longer unemployment spells. Thus, the eligibility screens appear to have directed demonstration services toward a population that generally faced reemployment difficulties. However, this was not the case for all demonstration-eligibles. Some were in the prime of their working lives, and some were individuals from industries (e.g., the service industry) that are strong and growing in New Jersey. Moreover, some were recalled by their pre-UI employers. Conversely, some claimants who were screened out appear, ex post, to have been good candidates for these special reemployment services.

Service Receipt

The demonstration achieved its objectives of providing an increased level of services to eligible claimants and of providing these services early in the unemployment spells of claimants. The three demonstration treatments offered claimants an identical set of initial job-search assistance services--orientation, testing, a job-search workshop, and an individual assessment/counseling interview--beginning in about the fifth week of their claim spells. These services were provided by ES staff in conjunction with JTPA staff. Three-quarters of the claimants in the treatment groups attended the orientation, and three-quarters of this group continued through the assessment/counseling interview. The level at which demonstration-eligible claimants received these services was substantially higher than the level at which individuals in the control group received these services from the existing service network.

Additional services were offered to claimants at the assessment/ counseling interview. These additional services differed by treatment group, but in all treatment groups the claimants were expected to maintain ongoing, periodic contact with demonstration ES staff as they searched for work. A set of up to five contact points was established, and ES staff were expected to call-in claimants who did not maintain contact. A large proportion of the individuals who continued to collect UI did maintain contact with the demonstration. While the rate of contact declined somewhat at the later contact points, the degree of contact was high relative to ongoing employment and training programs, which typically do not have systematic follow-up procedures. A resource center was also established in each office to provide job search materials (such as lists of job openings) and equipment (such as telephones) to assist claimants in their job search. These resource centers were not used uniformly among sites; they appeared to be used when staff promoted their use but not otherwise. Individuals in the first treatment group received these "additional" services only.

Individuals in the second treatment group were offered classroom training, on-the-job training, or relocation assistance by JTPA staff. About 15 percent of the claimants who were offered training participated in training, most of which was classroom training. While this rate of training receipt was low in absolute terms, it was higher than the rates observed for comparable groups of claimants whose exposure to training opportunities comes through the regular JTPA service environment in New Jersey. Over 60 percent of the training was provided in (1) business and office or (2) computer and information services, both of which represent areas in which employment prospects are strong in New Jersey. Several sites were considerably more successful than the others in placing individuals in training. Their success stemmed from a number of factors, including an early and enthusiastic presentation of the training option and an ability to offer a wide range of individual training slots. Finally, few individuals used the relocation assistance, as has been the experience in other demonstrations.

Individuals in the third treatment group were offered a reemployment bonus, which was larger the more quickly reemployment occurred. About 19 percent of the claimants who were offered the bonus received it. It appears that most claimants who were eligible for the bonus did in fact apply for the bonus.

The process of monitoring and enforcing claimants' compliance with the demonstration reporting requirements was accomplished through a reporting mechanism that was included as part of the tracking system developed for the project. This system provided a weekly report from ES to UI which identified the claimants who had not reported as scheduled for the initial sequence of services. The UI files of these claimants were marked, the reasons for noncompliance were examined when they claimed additional UI benefits, and they were referred back to demonstration services. While this process was complex and required close cooperation between UI and ES staff to work successfully, it played an important role in identifying claimants who had not complied with demonstration requirements. Some claimants who did not report for services did continue to collect UI benefits, but most of these individuals either had an eligibility determination or had some reason why a determination was not necessary.

Impacts on UI Receipt and Employment and Earnings

In general, the demonstration treatments were expected to hasten reemployment, thereby reducing the amount of UI collected. The potential exception was the JSA plus training/relocation treatment, for which short-run impacts on UI were expected to be lower than for the other treatments because individuals in training would be eligible to continue to collect benefits. Estimates of the impacts of the treatments on UI receipt show that all three treatments reduced the amount of benefits collected over the benefit year, by $87 per claimant for the first treatment, $81 for the second, and $170 for the third. These findings suggest that all the treatments were successful at reducing the time spent on UI, and that the bonus offer provided an extra incentive to become reemployed. Data on the timing of these impacts indicate that the rate at which individuals exited from the unemployment system increased primarily during the early part of their claim spells. This was during the period in which intensive job-search assistance was provided.

Evidence on the impacts of the treatments on employment and earnings indicates that all three treatments also increased employment and earnings in the year following the initial UI claim. These increases were larger in the first two quarters after the claim filing date than in the following two quarters, and larger for the JSA-only and JSA plus reemployment bonus treatments relative to the JSA plus training treatment. The training offer did not appear to have been a factor that contributed to the increases in employment and earnings while the reemployment bonus offer appeared to have had a small effect. Overall, however, these increases appear to have arisen primarily because the treatments promoted early reemployment through job-search assistance. This early reemployment did not entail any sacrifice in wages. In fact, the treatments appear to have led to modest increases in hourly wage rates on post-UI jobs.

Benefit-Cost Analysis

The benefit-cost analysis indicates that all three of the treatments offered net benefits to society as a whole and to claimants, when compared with existing services. The JSA-only and JSA plus reemployment bonus treatments also led to net gains to the government sector as a whole, although none of the treatments led to net benefits to the Labor Department agencies which actually offered the services. That is, the observed reductions in UI benefits paid to claimants did not by themselves outweigh the net cost of providing additional services. Overall, net benefits were similar for the JSA-only and JSA plus reemployment bonus treatments, while the JSA plus training/relocation treatment was more expensive than the other treatments from all perspectives.

Concluding Observations

Three additional evaluation findings should be noted. First, an important element of the treatments appears to have been the UI system requirement that claimants report for the initial job-search assistance services. Evidence from the evaluation suggests that the process of identifying and following-up with individuals who did not report and who continued to claim benefits was fairly successful. These reporting requirements and the compliance process were undoubtedly important factors that contributed to the impacts of the treatments on UI receipt and earnings.

Second, service delivery in the demonstration relied on the strengthening of linkages among the UI, ES, and JTPA systems, and these linkages appear to have occurred both centrally and, in most cases, at the local office level. This success required a high degree of central office supervision, which, we believe, would continue to be necessary in a future program.

Third, an examination of the impacts of the treatments by population subgroup suggests that the treatments were most successful at promoting the reemployment of individuals who had marketable skills, such as clerical and other white collar workers. The treatments were less successful for individuals who faced hard-core, structural unemployment problems, such as blue-collar workers, workers from durable-goods manufacturing industries, and permanently separated workers. That is, the displaced workers with more severe reemployment problems may have been less affected by the demonstration treatments than were other workers who faced relatively more favorable reemployment prospects. This finding suggests that the treatments, particularly the initial mandatory job-search assistance services, are appropriate and cost-effective for a broad-range of UI claimants who meet reasonable operational definitions of displacement, but that longer-run, more intensive services may be needed for displaced individuals who face major structural dislocations.

PART I

THE NEW JERSEY UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT

SUMMARY REPORT

Author:

Walter Corson

INTRODUCTION

The Unemployment Insurance (UI) system provides short-term income support to involuntarily unemployed individuals while they seek work. The UI system also often attempts to promote rapid reemployment by imposing various work-search requirements on UI claimants and by referring them to the Employment Service (ES) and, through the ES, to services offered under the Job Training Partnership Act (JTPA). However, a number of observers have proposed that more intensive services could appropriately be given to UI claimants to help them become reemployed. It has further been suggested that the more intensive reemployment assistance should be targeted toward permanently separated or displaced claimants who are expected to experience the greatest difficulty in becoming reemployed. It has also been argued that if reemployment assistance were provided early in the UI claim period the savings in UI benefit payments could potentially outweigh the costs of providing these services. In addition, even if paying for reemployment services for these workers does not prove cost-effective from the standpoint of UI, the UI system may play a socially important role by identifying a broad population of displaced workers early in their unemployment spells who could benefit from receiving the services.

The New Jersey Unemployment Insurance Reemployment Demonstration Project (NJUIRDP) was initiated by the United States Department of Labor (USDOL) through a cooperative agreement with the New Jersey Department of Labor (NJDOL) to test whether the UI system can be used to identify displaced workers early in their unemployment spells and to test alternative early intervention strategies to accelerate their return to work. Three packages of services, or treatments, were tested in the demonstration: (1) job-search assistance only, (2) job-search assistance combined with training or relocation assistance, and (3) job-search assistance combined with a cash bonus for early reemployment. A key component of the demonstration was that eligible claimants were identified and services were provided through the coordinated efforts of the Unemployment Insurance (UI), Employment Service (ES), and Job Training Partnership Act (JTPA) systems.[1] Another key component was that claimants were required by UI to report for services; failure to report could have led to the denial of benefits.

---

[1] The first two treatment packages and the emphasis on interagency cooperation and coordination are similar to provisions contained in the recently enacted Economic Dislocation and Worker Adjustment (EDWAA) program.

The demonstration was initiated in July 1986, and, by the end of sample selection in June 1987, 8,675 UI claimants were offered one of the three service packages. Services to eligible claimants were continued into fall 1987 to ensure that all eligibles were able to receive, if desired, the full set of demonstration services. Another 2,385 claimants were randomly selected to provide a control group for comparative purposes for the evaluation. These claimants received existing services. During the demonstration period, the New Jersey economy was experiencing worker displacement generated by a long-term secular decline in manufacturing, while substantial growth was occurring in other sectors. Overall, the state economy was quite strong, and the unemployment rate during the period was low (5 percent).

The evaluation of the demonstration consists of two main components: (1) an impact and benefit-cost report (Corson, Decker, and Gordon, 1989) and (2) an implementation and process report (Corson and Dunstan, 1989). This summary paper presents the main findings of these two reports.

OVERVIEW OF THE DEMONSTRATION DESIGN

The NJUIRDP was designed to address three objectives: (1) to examine the extent to which UI claimants who could benefit from the provision of employment services can be identified early in their unemployment spells; (2) to assess the policies and adjustment strategies that are effective in helping such workers become reemployed; and (3) to examine how such a UI reemployment program should be implemented. To achieve these objectives, the design called for identifying demonstration-eligible individuals in the week following their first UI payment, and assigning eligible individuals randomly to three treatment groups that were offered alternative packages of reemployment services, and to a control group that received existing services. The demonstration was implemented in 10 sites, which corresponded to state UI offices. The sites were chosen randomly, with the probability of their selection proportional to the size of the UI population in each office.

Definition of Eligibility. The purpose of the demonstration was to provide reemployment services to experienced workers who, having become unemployed through no fault of their own, were likely to face prolonged spells of unemployment. Their job-finding difficulties might be due to the unavailability of jobs, a mismatch between their skills and job requirements, or their lack of job-finding skills. However, because previous research efforts had failed to establish good predictors of prolonged unemployment spells (see, for example, Crosslin, Hanna, and Stevens, 1984), complex eligibility requirements could not be used to channel demonstration services. Thus, one objective of the demonstration research was to further investigate the possible predictors that could be applied in future programs.

Faced with this objective, the demonstration plan incorporated a small number of sample screens which were chosen to identify experienced workers who were likely to be displaced permanently from their jobs. More complex screens were to be evaluated by examining the effects of the demonstration on alternatively defined samples.

The following eligibility screens were chosen for the demonstration:

1. First Payment. The demonstration excluded claimants who did not receive a first UI payment. To promote early intervention, the demonstration also excluded claimants who did not receive a first payment within five weeks after the initial claim. Individuals who were working and, consequently, who received a partial first payment were also excluded, since their job attachment meant that they had not necessarily been displaced. Finally, claims of a "special" nature (e.g., unemployment compensation for ex-servicemembers, unemployment compensation for federal civilian employees, interstate claims, combined wage claims, etc.) were also excluded.

2. Age. An age screen was applied to eliminate the broad category of young workers who have traditionally shown limited attachment to the labor market and whose employment problems may be quite different from older, experienced workers. This screen was set so that workers younger than 25 years of age were excluded from the demonstration.

3. Tenure. It was decided that demonstration-eligible claimants should have exhibited a substantial attachment to a job (or at least to have worked) so that the loss of a job was associated with one or more of the reemployment difficulties described earlier in this section. This decision was implemented by requiring each claimant to have worked for his or her last employer for three years prior to applying for UI benefits and not to have worked full-time for any other employer during the three-year period. The three-year requirement is used by USDOL's Bureau of Labor Statistics to define dislocated workers (Flaim and Sehgal, 1985).

4. Temporary Layoffs. The demonstration treatments were not intended for workers who were facing only temporary layoffs. Thus, it was desirable that claimants on temporary layoff be excluded. However, previous research and experience show that many individuals report that they expect to be recalled even when their chances of actual recall are slim. In order not to exclude such individuals from demonstration services, only individuals who both expected to be recalled and had a specific recall date were excluded.

5. Union Hiring-Hall Arrangement. Individuals who are typically hired through union hiring halls exhibit a unique attachment to a specific labor market and were thus excluded from the demonstration.

Treatments. As stated earlier, the demonstration tested three treatment packages for enhancing reemployment. Eligible claimants were assigned randomly to the three treatment groups (job-search assistance (JSA) only, JSA plus training or relocation, and JSA plus a reemployment bonus) and to a control group which received services that were then currently available.

All three treatments began with a common set of initial components (notification, orientation, testing, a job-search workshop, and an assessment/counseling interview), which were delivered sequentially early in the claimants' unemployment spells. First, a notification letter was sent to claimants after they received their first payment, which occurred about the third week after they filed their claims; thus, claimants usually began to receive services during their fifth week of unemployment. These services began when they reported to a demonstration office (usually an ES office) and received orientation and testing during the same week. In the following week, they attended a job-search workshop consisting of five half-day sessions, and a follow-up, one-on-one counseling/assessment session scheduled for the subsequent week. These initial treatment components were mandatory; failure to report could have led to the denial of UI benefits.

Beginning with the assessment/counseling interview, the nature of the three treatments differed. In the first treatment group--job-search assistance (JSA) only--claimants were told that as long as they continued to collect UI they were expected to maintain periodic contact with the demonstration office, either directly with staff to discuss their job-search activities or by engaging in search-related activities at a resource center situated in the office. The resource center contained job-search materials and equipment, such as job listings, telephones, and occupational and training literature. Claimants were encouraged to use the resource center actively, and were told that if they did not come to the office periodically they would be contacted by ES staff and asked to do so. These periodic follow-up contacts were to occur at 2, 4, 8, 12, and 16 weeks following the assessment interview. ES staff were expected to notify UI when a claimant did not report for services.

Claimants in the second treatment group--JSA plus training or relocation--were also informed about the resource center and of their obligation to maintain contact during their job-search period. In addition, they were told about the availability of classroom and on-the-job training, and they were encouraged to pursue training if interested. Staff from the local JTPA Service Delivery Area (SDA) program operator worked directly with these claimants to pursue the training options. These claimants were also told about the availability of relocation assistance, which, if they elected not to pursue training, they could use to pay for out-of-area job search and for moving expenses.

Claimants in the third treatment group--JSA plus a reemployment bonus--were offered the same set of JSA services as was the first treatment group, but also a bonus for rapid reemployment. The maximum bonus equalled one-half of the claimant's remaining UI entitlement at the time of the assessment interview. This amount was available to the claimant if he or she started work either during the assessment week or in the next two weeks. Thereafter, the potential bonus declined at a rate of 10 percent of the original amount per week until it was no longer available. Claimants could not receive a bonus if they were recalled by their former employer, if the job was with a relative, or if the job was temporary, seasonal, or part-time. They received 60 percent of the bonus if they were employed for 4 weeks, and the remainder if they were employed for 12 weeks. The bonus was expected to provide a strong incentive to the claimant to engage in early, intensive job-search.

Each of these treatments tested a different view of the employment problems faced by displaced workers. More specifically, the JSA-only treatment was based on the assumption that many displaced workers have marketable skills but do not have sufficient job-search experience to identify these skills and sell them in the job market. In contrast, the training treatment was based on the assumption that the skills of some workers are outmoded and must be upgraded. Finally, the reemployment bonus treatment was based on the assumption that JSA alone is an insufficient incentive for claimants to obtain employment rapidly, and that an additional incentive will help them recognize the realities of the job market and accept a suitable job more rapidly than would otherwise be the case.

With the exception of the reemployment bonus and relocation assistance, the services that were offered in the demonstration are similar to those that were available under the existing ES and JTPA systems in New Jersey. However, the likelihood that a claimant was offered and received these services in the demonstration was considerably greater than under the existing system. Moreover, the timing of service receipt also differed: demonstration services were generally provided earlier in the unemployment spell than were existing services.

Provision of Demonstration Services. An important objective of the demonstration was to examine how a reemployment program targeted toward UI claimants should be implemented. Two aspects of that objective were given considerable emphasis in the demonstration design phase: (1) using existing agencies and vendors to provide the services, and (2) using a computer-based participant tracking system to facilitate the delivery of services.

In the NJUIRDP, the first aspect meant that the UI agency, the ES, and JTPA's local program operators were all involved in delivering services, and that strengthening linkages among these agencies was an important component of the demonstration. UI staff were responsible for collecting the data that were used to select eligible claimants, and for monitoring compliance by claimants with the demonstration's reporting requirements. A determination of UI eligibility was to be performed when claimants did not report for the initial mandatory services, and, if appropriate, benefits were to be denied.

The initial reemployment services, together with the additional services offered at the assessment/counseling interview, were provided in each demonstration office by a four-person team. This team consisted of three ES staff members and a JTPA staff member from the local SDA program operator. An ES counselor was the team leader and had overall responsibility for ensuring the provision of services. ES staff provided all of the services for the JSA-only and JSA plus reemployment bonus treatment group members. JTPA staff members were involved only with the JSA plus training/relocation treatment group members. They were expected to become involved with the claimants during the assessment/counseling interview and to work with individuals who were interested in classroom or on-the-job training to identify appropriate opportunities and to place the claimants in them. The goal was to use the training opportunities available in each local JTPA SDA. Thus, this component of the demonstration strengthened the linkages between the ES and the local JTPA program operators in the ten demonstration sites.

The other important aspect of the implementation of the demonstration was the extensive use of a computer-based tracking system to operate the program. Data on service delivery were entered into the system, and local office staff were provided with weekly lists of claimants who were expected to receive services. A list of claimants who did not report for services was also generated for use by UI, and monitoring reports were provided to central office staff. The system helped ensure that services were delivered as specified, and that claimants were not "lost" from the program.

EFFECTIVENESS OF THE ELIGIBILITY DEFINITION

The eligibility requirements targeted demonstration services toward about one-quarter of the claimants who received a first UI payment. A first round of exclusions was made on the basis of routinely collected UI agency data. This pass-through of the records of all claimants who received a first payment excluded about 28 percent of the claimants, with the most important screen being the age restriction that excluded claimants younger than age 25.

The remainder of the eligibility screening was implemented with data collected by UI staff specifically for the demonstration. The most restrictive screen applied at this point was the tenure requirement, which excluded individuals who had not worked for their pre-UI employer for three years previously. This requirement excluded about half of the claimants who passed the mainframe eligibility screens.

The other important eligibility requirement that merits discussion is the temporary layoff screen, which excluded claimants with a definite recall date. This screen excluded about 13 percent of the claimants who survived the initial examination of agency data. In devising this screen, it was decided that establishing some evidence that the layoff was indeed temporary was necessary, rather than relying solely on the claimant's expectation. Having a definite recall date was used for this purpose. As expected, however, a substantially larger percentage of claimants said that their layoff was temporary than the number who actually had a recall date. About half of the claimants who expected to be recalled but who had no recall date did return to their pre-UI job.

The eligibility definition was designed to identify claimants who, in the absence of demonstration services, would experience difficulty in becoming reemployed. An examination of the characteristics of the eligible population shows that it contained a substantial proportion of individuals whose age, industry of employment, and other characteristics are usually associated with the displaced worker population and with difficulties in becoming reemployed. Moreover, compared with a sample of individuals who were not eligible for the demonstration, the eligible population experienced considerably longer periods of UI collection and longer unemployment spells on average. Thus, the eligibility screens appear to have directed demonstration services to a population that generally faced reemployment difficulties. However, it is unlikely that all demonstration eligibles required services. Some were in the prime of their working lives and some were individuals from industries (e.g., the service industry) that are strong and growing in New Jersey. Moreover, some were recalled by their pre-UI employers.

THE RECEIPT OF INITIAL SERVICES

All claimants who were selected as demonstration treatment group members were offered a common set of reemployment services early in their UI claim period. These services occurred in sequence and consisted of orientation, testing, a job-search workshop, and an assessment/counseling interview.

Service Receipt. Data on the receipt of these initial services (see Table 1) show that 77 percent of the selected claimants attended orientation as requested. Most attended their scheduled session, but some attended a later session, generally after questioning by the UI claims examiner. Three-quarters of the claimants who attended orientation continued through the assessment/counseling interview.

TABLE 1

RECEIPT OF THE INITIAL REEMPLOYMENT SERVICES

	Total
As Percentage of the Total Sample	
Attended Orientation	
Scheduled orientation	67.9
Later orientation	8.9
Total	76.8
Tested	45.5
Excused from Testing[a]	28.4
Completed JSW[b]	49.8
Excused from JSW	19.8
Attended Assessment/Counseling Interview	56.2
As Percentage of Those Attending Orientation	
Tested	59.2
Excused from Testing	37.0
Completed JSW	64.8
Excused from JSW	25.8
Attended Assessment/Counseling Interview	73.2
Sample Size	8,675

[a] Includes 0.2 percent who were excused because they had previously been tested by the ES.

[b] Includes 0.5 percent who were excused because they had already completed a job-search workshop.

However, not all such individuals were tested or attended a job-search workshop. Some individuals were excused from all services, generally because their recall expectations could be substantiated. A substantial number of others were excused specifically from testing and the workshop because of language or reading comprehension difficulties (which precluded testing). This situation suggests that programs might want to emphasize referrals to English as a Second Language courses or remedial education for such individuals as part of such an early orientation and screening process.

The Timing of Service Receipt. Most claimants attended orientation during the fifth week after their UI claim, and most completed assessment over the following three- to four-week period. Thus, the goal of early intervention was achieved as planned. This orientation might be accelerated if data to make the eligibility determination were collected as part of the UI application process and if eligibility determination was accomplished at that point.

Comparison with the Existing Service System. The level at which treatment group members received the initial services--testing, job-search workshops, and counseling--substantially exceeded the level at which control group members received such services from ES and JTPA through existing referral mechanisms. Thus, the demonstration achieved its objective of increasing the level of job-assistance service receipt by eligible claimants.

THE RECEIPT OF ADDITIONAL SERVICES

The additional services that were offered to claimants at the assessment/counseling interview included the periodic JSA activities, training and relocation assistance, and the reemployment bonus.

JSA Follow-Up. The objective of the follow-up activities was to encourage on-going, intensive job search by all claimants, except those in the second treatment who were engaged in training. This intensive job-search was to be promoted by disseminating job-search materials at the resource centers and by requiring that claimants maintain periodic contact with demonstration staff, either through the resource centers or directly in person.

Data on claimants who were collecting UI at the five targeted follow-up points (2, 4, 8, 12, and 16 weeks after assessment) show that 92 percent satisfied the first follow-up requirement (i.e., the 2-week contact), and 80 percent had a contact at 16 weeks. Although the rate of contact declined somewhat at the later contact points, the degree of contact was high relative to ongoing employment and training programs, which typically do not have systematic follow-up procedures. However, these periodic contacts did not always follow the strict schedule that had been laid out in the design, nor were all the contacts made in-person as desired. In addition, the resource centers appear to have been used fairly extensively in only a few of the offices, and consequently, the use of these centers probably had, at most, a minor impact on demonstration outcomes.

Training and Relocation Assistance. Classroom and on-the-job (OJT) training opportunities were offered to claimants in the second treatment to test the efficacy of a service package that, early in the unemployment spell, attempts to alter or upgrade the skills of individuals whose current set of job skills are no longer in demand.[2] About 15 percent of the claimants who were offered training participated in training, most of which was classroom training. Much of the classroom training was in business and office services or computer and information services, while the OJT tended to be in technical, clerical, and sales occupations. Thus, it appears that the training that was offered was directed toward occupations whose employment prospects were strong in New Jersey.

The rate of training receipt was higher than the rate observed for comparable groups of claimants whose exposure to training opportunities came through the regular JTPA service environment in New Jersey. Thus, the offer of training under the demonstration did appear to increase the receipt of training as designed. Nevertheless, the overall rate of training receipt was lower than initially expected, based on the training participation rate among individuals who participate in JTPA Title III and in other dislocated worker programs.

Two general reasons appear to explain the lower-than-expected increase in training participation. First, the nature of the training intervention differed from that which is offered by other programs. The offer occurred early in the layoff period, which may have been before many individuals were ready to accept the fact that an occupational change was necessary. Moreover, not all individuals who were offered training were interested in (or needed) any reemployment services, let alone training, but were offered services due to the mandatory nature of the initial services.

The second reason that training participation was lower than might have been expected pertains to the implementation of the demonstration. The training treatment relied on the existing JTPA local program operators to provide the training placement function, and some operators were considerably more successful than others at placing claimants in training. Their success stemmed from a number of factors, including an early and enthusiastic presentation of the training option and an ability to offer a wide range of individual training slots.

---

[2] Individuals in this treatment group were also offered relocation assistance. As previous experience suggested, few individuals were interested in relocation, and fewer than one percent of those who were offered relocation assistance received it.

The Reemployment Bonus. The third treatment package included a reemployment bonus that was offered to claimants at the assessment/counseling interview. The purpose of the reemployment bonus was to provide a direct financial incentive for claimants to seek work actively and become reemployed. The full bonus offer averaged $1,644 and was paid for jobs that started by the end of the second full week following the interview. After that point, it declined by 10 percent of the initial amount each week, so that it fell to zero by the end of the eleventh full week of the offer.

Nineteen percent of the claimants who were offered the bonus received a first bonus payment, which was paid to individuals who held a bonus-eligible job for at least four weeks. Eighty-four percent of this group also received the final bonus payment, which was paid after 12 weeks of work. Overall, the total of the two bonus payments averaged close to $1,300 for those who received them.

About 30 percent of the claimants who were offered a bonus began a job within the bonus period, compared with the 19 percent who received a bonus. The remaining 12 percent appeared largely to be ineligible for the bonus, primarily because they obtained a job with their pre-UI employer: claimants who returned to their pre-UI employers were not eligible for the bonus.

IMPACTS OF THE DEMONSTRATION TREATMENTS ON UI RECEIPT

The demonstration treatments were expected to affect the receipt of UI benefits by eligible claimants. The JSA-only and JSA plus reemployment bonus treatments (the first and third treatments) were expected to help eligible claimants become reemployed rapidly, thereby reducing the amount of UI benefits received by treatment group members relative to the amount received by control group members; further, the JSA plus reemployment bonus treatment was expected to have a larger impact on UI receipt because of the reemployment incentives created by the bonus. Expectations about the JSA plus training or relocation treatment on short-run UI receipt were less certain. Individuals in this treatment who did not receive training were expected to experience a reduction in UI receipt, but those who entered training were expected to experience an increase in receipt, since individuals who accepted training continued to collect UI.

Estimates of the impacts of the treatments on UI receipt (Table 2) show that all three treatments did reduce the amount of benefits collected over the benefit year, by $87 for the first treatment, $81 for the second, and $170 for the third. As expected, these impacts were largest for the third

TABLE 2

ESTIMATED TREATMENT IMPACTS ON UI RECEIPT

	JSA Only	JSA Plus Training/Relocation	JSA Plus Reemployment Bonus
Dollars Paid in Benefit-Year	-87*	-81*	-170***
Weeks Paid in Benefit-Year	-0.47*	-0.48**	-0.97***
Weeks Paid in First Spell	-0.59**	-0.53**	-0.93**
Exhaustion Rate	-0.028**	-0.017	-0.037***

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

treatment--JSA plus the reemployment bonus. As shown in the table, these impacts were mirrored in other measures of UI receipt, such as weeks collected and the exhaustion rate. The fact that the exhaustion rate showed a decline is important because it indicates that the treatments affected some claimants who, in absence of the treatments, would have experienced long spells of UI receipt. An examination of data on the timing of these impacts indicate that the rate at which individuals exited from the unemployment system increased during the early part of their claim spells, which was during the period in which intensive job-search assistance was provided.

THE IMPACTS OF THE DEMONSTRATION TREATMENTS ON EMPLOYMENT AND EARNINGS

In general, the treatments were expected to promote the rapid reemployment of claimants, and thus to have a positive impact on the employment and earnings of claimants following their entry into the UI system. As noted in the discussion on UI receipt, short-run impacts were expected to be greater for the JSA-only and JSA plus reemployment bonus treatments than for the JSA plus training treatment, since individuals who entered training were expected to sacrifice short-run earnings for longer-run earnings gains.

Estimates of the impacts of the treatments on employment and earnings (Table 3) indicate that all three treatments increased employment and earnings in the year following the initial UI claim.

TABLE 3

ESTIMATED TREATMENT IMPACTS ON EMPLOYMENT, EARNINGS, AND POST-UI WAGES

	JSA Only	JSA Plus Training/Relocation	JSA Plus Reemployment Bonus
Percent of Time Employed			
Quarter 1	2.3**	1.9**	2.8***
Quarter 2	4.2***	2.8*	5.0***
Quarter 3	4.3**	2.2	2.3
Quarter 4	2.8	1.7	0.6
Earnings			
Quarter 1	$125**	$82	$160***
Quarter 2	263**	103	278***
Quarter 3	171	83	131
Quarter 4	49	77	22
Percent Change in Post-UI Relative to Pre-UI Hourly Wage	0.041**	0.030**	0.041**

NOTE: Quarters are defined relative to the UI date of claim. That is, quarter 1 is the first three months following the date of claim, quarter 2 is the next three months, and so on.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

These increases were larger in the first two quarters after the claim filing date than in the following two quarters, and, as expected, were larger for the JSA-only and JSA plus reemployment bonus treatments relative to the JSA plus training treatment. The training offer did not appear to have been a factor that contributed to the increase in employment and earnings, while the reemployment bonus offer appeared to have had a small effect. Overall, these impacts appear to have arisen primarily because the treatments promoted early reemployment through job-search assistance.

Since the impacts of training receipt were expected to occur in the longer-run, the impacts for the fifth and sixth quarters following the claim filing date were also investigated. This examination showed that the JSA plus training treatment had no impacts on employment or earnings in these quarters. However, since relatively few individuals in the JSA plus training treatment actually received training, and since sufficient time had not elapsed to observe post-training employment outcomes for all these individuals, these findings should be considered inconclusive as they pertain to the value of training per se for the demonstration-eligible population.

A final employment and earnings issue that was investigated was the impact of the treatments on the characteristics of the first post-UI job. This is an important issue, since it is possible that, by promoting rapid reemployment, the treatments might have prompted claimants to accept jobs that were less desirable than those obtained by claimants who were not offered special services. An examination of this issue indicates that the early reemployment promoted by the treatments did not entail any sacrifice in hourly wages or hours worked. In fact, the treatments appear to have led to modest increases in hourly wage rates in post-UI jobs.

BENEFIT-COST ANALYSIS

An important question for any potential program or policy is whether the benefits of offering services exceed their costs. This question was examined for the three treatments tested in the demonstration by examining benefits and costs from the perspective of claimants, the government, and society as a whole.[3] For example, the reductions in UI benefit receipt represent a cost to claimants, a benefit to the government, and neither a benefit nor a cost to society, since UI payments are transfers from one sector of society to another. The analysis considered net benefits (including gains in earnings and taxes paid) and net costs relative to the existing service system.

---

[3] The perspective of employers was also examined, and it was concluded that benefits were likely to equal costs from the employer perspective.

In terms of costs, it was estimated that the gross costs of providing the three treatments were $169 per claimant for the JSA-only treatment, $491 per claimant for the JSA plus training or relocation treatment, and $300 per claimant for the JSA plus reemployment bonus treatment. Because some reemployment services are already provided to UI claimants under the existing service system, the net cost of providing these treatments was lower: $155 for the first treatment, $377 for the second treatment, and $277 for the third treatment.

The results of the benefit-cost analysis (Table 4) indicated that each of the treatments offered net benefits to society as a whole and to claimants when compared with existing services.[4] The JSA-only and JSA plus reemployment bonus treatments also led to net gains to the government sector as a whole, but not to the Labor Department agencies which actually offer the services. That is, by themselves, the reductions in UI benefits did not outweigh the net cost of providing additional services to claimants.[5] Overall, net benefits were similar for these two treatments, while the JSA plus training/relocation treatment was more expensive than the other treatments from all perspectives.

SUMMARY

The demonstration showed that the treatments tested in the demonstration could be implemented successfully. That is, eligible claimants can be identified and provided with services early in their unemployment spell through the coordinated efforts of the UI, ES, and JTPA systems. Moreover, each of the treatments did lead to reductions in the lengths of unemployment spells and to concomitant increases in earnings and reductions in UI benefits received. All three of the treatments offered net benefits to society as a whole, and to claimants, when compared with existing services. The JSA-only and JSA plus reemployment bonus treatments also led to net gains to the government sector as a whole, although none of the treatments led to net benefits to the Labor Department agencies which actually offered the services.

These overall, generally positive findings suggest that the demonstration treatments represent potentially useful reemployment policies that could be directed toward UI claimants. However, several

---

[4] The net benefits to society occur largely because it is assumed that the increased employment and earnings experienced by claimants represent a net increase in output. That is, it is assumed that the more rapid reemployment of claimants did not displace the employment of other individuals. This no-displacement assumption seems reasonable given the strength of the New Jersey economy.

[5] Increased tax collections arising from claimants' increased earnings were assumed to accrue to the government as a whole, but only a small portion was assumed to accrue to Labor Department agencies.

TABLE 4

BENEFIT-COST COMPARISON WITH EXISTING SERVICES
(Dollars per Claimant)

Perspective	JSA Only	JSA Plus Training/Relocation	JSA Plus Reemployment Bonus
Society	581	44	565
Claimants	493	258	510
Government	88	-214	55
Labor department	-61	-291	-99
Other government	149	78	154

NOTE: Entries are the sum of benefits minus costs.

further evaluation findings should be considered in any future implementation: the targeting of services, the application of participation requirements, efforts at promoting interagency coordination, and the selection of reemployment services.

Targeting Services. An important question for any reemployment strategy is, to whom should services be provided? The eligibility definition used in the demonstration attempted to target services toward displaced workers who would experience reemployment difficulties. In general, this objective was achieved, although some individuals selected for the demonstration presumably did not need services since they were eventually recalled by their former employers. The remainder covered the spectrum of permanently separated workers, from those who had marketable skills and needed few, if any, services to those who faced major reemployment difficulties.

The analysis of the impacts of the treatments by population subgroup suggests that the treatments were most successful at promoting the reemployment of the individuals who had marketable skills, such as clerical and other white-collar workers. The treatments were less successful for individuals facing hard-core, structural unemployment problems, such as blue-collar workers, workers from durable-goods manufacturing industries, and permanently separated workers. That is, the displaced workers with more severe reemployment problems may have been affected less by the demonstration treatments than were other workers who faced relatively more favorable reemployment prospects. This finding suggests that the treatments, particularly the initial mandatory job-search assistance services, are appropriate and cost-effective for a broad-range of UI claimants who meet reasonable operational definitions of displacement, but that longer-run, more intensive services are needed for displaced individuals who face major structural dislocations. The demonstration did offer occupational training in the second treatment (see further below), but additional services may be needed. For example, the high rate of excusals from testing and the job-search workshop for language and literacy reasons suggests that referrals to English as a Second Language or remedial education services may be needed for some individuals.

The Application of Participation Requirements. An important element of the treatments appears to have been the UI system requirement that claimants report for the initial job-search assistance services. Moreover, evidence from the evaluation suggests that this requirement was successfully implemented by UI and ES staff. That is, individuals who did not report and who continued to claim benefits were, in most cases, identified and followed-up. Thus, these reporting requirements and the compliance process were probably important factors that contributed to the increase in service receipt and to the impacts of the treatments on UI receipt and earnings.

Promoting Interagency Coordination. An important element of the New Jersey demonstration was that it relied on the coordinated efforts of the UI, ES, and JTPA systems to identify eligible claimants and to provide them with services. To be successful, such coordination required that linkages among these agencies be strengthened at both the local service delivery level and the central office level. These linkages appear to have been developed in the New Jersey demonstration; staff at both levels were enthusiastic and worked well together. This success, however, required a high degree of involvement and supervision by central office staff, which would also be necessary in any future program.

Service Selection. The findings summarized earlier indicate that the job-search assistance component of the treatments was successful at promoting the reemployment of claimants. In particular, the UI and earnings impacts appear to have occurred early in individuals' claims spells, a time period in which intensive job-search assistance was provided. The benefit-cost analysis also indicated that the JSA-only treatment generated a net social benefit.

The findings also indicated that the addition of the training or relocation assistance offer to the basic job-search assistance services did not lead to larger short-run impacts. In fact, as could be expected, the impacts were slightly smaller, because individuals who entered training continued to collect UI and delayed their return to employment. Moreover, since the cost of training itself was high, the training treatment was expensive relative to the other treatments, even though only a small percentage of individuals received training. However, these findings should not be viewed as indicating that training should not be offered. Training could have longer-run impacts that have not been measured in this study, and such longer-run impacts may be valuable for the individuals without marketable skills on whom the treatments had little short-run impact.

The findings on the reemployment bonus offer showed that the amount of UI benefits received by claimants who were offered the bonus was significantly less than the amount received by claimants who were not offered the bonus. Employment and earnings differences between those who were offered the bonus and those who were not were positive for the first two quarters after the claim filing date, but these differences did not persist into later quarters. Only the first quarter impact was statistically significant. Nevertheless, this finding together with the findings concerning UI receipt suggest that the bonus offer helped hasten the reemployment of claimants. However, the benefit-cost analysis indicated that the additional UI savings generated by the bonus offer did not offset the cost of the bonus itself, nor were the gains in earnings sufficiently greater than those obtained from the JSA-only treatment to make a difference in the benefit-cost comparisons. Overall, the JSA-only and JSA plus reemployment bonus treatments had very similar benefit-cost outcomes from all perspectives. Thus, the results from the New Jersey demonstration suggest that a reemployment bonus offer does not appear to improve labor-market outcomes sufficiently to make the combination of mandatory job-search assistance plus the bonus offer a more successful treatment than mandatory job-search assistance alone. An unanswered question is how a bonus offer by itself would compare with job-search assistance alone, particularly if job-search assistance contains mandatory elements, as was the case in New Jersey. Two other demonstrations, in Pennsylvania and Washington, are exploring a wide-range of reemployment bonus plans.

REFERENCES

Corson, W., P. Decker, and A. Gordon. "Impact and Benefit-Cost Report." Part 3 in The New Jersey Unemployment Insurance Reemployment Demonstration Project Final Evaluation Report. Princeton, NJ: Mathematica Policy Research, Inc., April 1989.

Corson, W., and S. Dunstan. "Implementation and Process Report." Part 2 in The New Jersey Unemployment Insurance Reemployment Demonstration Project Final Evaluation Report. Princeton, NJ: Mathematica Policy Research, Inc., April 1989.

Crosslin, R., J. Hanna, and D. Stevens. "Using Unemployment Insurance Data to Predict Economic Hardship from Displacement." In Displaced Workers: Implications for Educational and Training Institutions, edited by K. Hollenbeck, F.C. Pratzner, and H. Rosen. Columbus, OH: The National Center for Research in Vocational Education, The Ohio State University, 1984.

Flaim, P.O., and E. Sehgal. "Displaced Workers of 1979-83: How Well Have They Fared?" Monthly Labor Review, vol. 108, no. 6, June 1985, pp. 3-16.

PART 2

THE NEW JERSEY UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT

IMPLEMENTATION AND PROCESS REPORT

Authors:
Walter Corson
Shari Miller Dunstan

CONTENTS

Chapter Page

I INTRODUCTION 29
A. PURPOSE AND OBJECTIVES OF THE REPORT 30
B. OVERVIEW OF THE DEMONSTRATION DESIGN 31
1. Definition of Eligibility 31
2. Treatments 32
3. Provision of Demonstration Services 34
C. INFORMATION SOURCES 35
D. OUTLINE OF THE REPORT 36

II THE LOCAL ENVIRONMENTS 37
A. SITE SELECTION 37
B. THE CHARACTERISTICS OF THE SITES 38
1. Paterson and Hackensack (Part of the Bergen-Passaic Labor Market Area) 40
2. Jersey City (Part of the Jersey City Labor Market Area) 41
3. Butler, Bloomfield, Newark, and Elizabeth (Part of the Newark Labor Market Area) 42
4. Perth Amboy (Part of the Middlesex-Somerset-Hunterdon Labor Market Area) 43
5. Burlington and Deptford (Part of the Camden Labor Market Area) 43
C. CHARACTERISTICS OF THE ELIGIBLE POPULATION 44
D. SUMMARY 46

III IDENTIFYING AND NOTIFYING THE ELIGIBLE POPULATION 47
A. IDENTIFYING THE ELIGIBLE POPULATION 47
B. ASSIGNMENT TO AND NOTIFICATION OF TREATMENT STATUS . . . 54
C. IMPORTANCE OF THE FACTORS THAT AFFECT ELIGIBILITY 56
D. THE CHARACTERISTICS OF THE ELIGIBLE POPULATION 60

IV THE INITIAL SET OF COMMON SERVICES 69
A. THE INITIAL MANDATORY SERVICES 69
1. Orientation 69
2. Testing 71
3. The Job-Search Workshop 72
4. Assessment/Counseling 72
5. Summary of Common Elements 74
B. PARTICIPATION IN MANDATORY SERVICES 74
1. Mandatory Service Receipt 74
2. The Timing of Service Delivery 79
C. ASSESSMENT OF THE MANDATORY SERVICES 82
1. Excusal Policies 82
2. Testing 84
3. Job Search Workshop 84
4. Assessment/Counseling Interview 86
5. Conclusions 87

V PERIODIC JOB SEARCH ASSISTANCE 89
A. THE RESOURCE CENTER AND JSA FOLLOW-UP ACTIVITIES 89
B. EXTENT OF JSA FOLLOW-UP 91
C. THE NATURE OF THE JSA FOLLOW-UP 98
D. SUMMARY AND ASSESSMENT 101

CONTENTS (continued)

Chapter Page

VI TRAINING AND RELOCATION 103
A. THE TRAINING OFFER AND PLACEMENT PROCESS 103
B. TRAINING PARTICIPATION AND THE NATURE OF THE TRAINING 107
C. ASSESSMENT OF THE TRAINING OFFER AND PLACEMENT PROCESS 113
1. The NJUIRDP Intervention 113
2. Implementation of the Training Offer and Placement Process 115
D. RELOCATION ASSISTANCE 118

VII THE REEMPLOYMENT BONUS 121
A. THE REEMPLOYMENT BONUS OFFER 121
B. THE PAYMENTS MECHANISM 123
C. RECEIPT OF THE BONUS 127

VIII MONITORING COMPLIANCE 131
A. THE MONITORING AND COMPLIANCE PROCESS 131
B. EVIDENCE ON THE DEGREE OF MONITORING AND COMPLIANCE 136
C. ASSESSMENT OF THE MONITORING AND COMPLIANCE PROCESS 142

IX ORGANIZATION AND STAFFING 145
A. ORGANIZATIONAL STRUCTURE 145
B. STAFF TRAINING 150
C. ORGANIZATIONAL AND STAFFING ISSUES 151

X COSTS OF THE INTERVENTION 155
A. ESTIMATING PROGRAM COSTS 155
1. Treatment Payments 156
2. Estimating Operational Costs 157
3. Per-Claimant Expenditures by Treatment Intervention 161
B. TREATMENT PAYMENTS AND OPERATIONAL COSTS PER CLAIMANT 162
C. THE SENSITIVITY OF THE ESTIMATES TO ALTERNATIVE ASSUMPTIONS 164
D. SUMMARY AND CONCLUSIONS 168

XI REPLICABILITY AND SUMMARY 171

REFERENCES 175

LIST OF TABLES

Table		Page
II.1	EMPLOYMENT CHARACTERISTICS OF THE LABOR MARKET AREAS AND COUNTIES WITHIN WHICH DEMONSTRATION LOCAL OFFICES WERE LOCATED	39
II.2	CHARACTERISTICS OF THE NJUIRDP ELIGIBLE POPULATION, BY OFFICE (Percent)	45
III.1	MATCHING OF FIRST PAYMENTS WITH NEW CLAIMANT QUESTIONNAIRE DATA, BY QUARTER OF SAMPLING	51
III.2	MATCHING OF FIRST PAYMENTS WITH NEW CLAIMANT QUESTIONNAIRE DATA, BY LOCAL OFFICE	53
III.3	DISTRIBUTION OF NJUIRDP ELIGIBLE CLAIMANTS BY TREATMENT, CONTROL STATUS AND OFFICE	55
III.4	IMPACT OF THE ELIGIBILITY SCREENS ON FIRST PAYMENTS UNDER THE REGULAR STATE UI PROGRAM, BY LOCAL OFFICE	58
III.5	CHARACTERISTICS OF THE ELIGIBLE AND NONELIGIBLE POPULATION	62
III.6	COMPARISON OF THE UI AND LABOR MARKET EXPERIENCE OF ELIGIBLES AND NONELIGIBLES	66
IV.1	RECEIPT OF THE INITIAL REEMPLOYMENT SERVICES, BY OFFICE (Percent)	75
IV.2	THE TIMING OF THE INITIAL SERVICES, BY OFFICE (Percent)	80
V.1	ASSESSED CLAIMANTS WHO WERE EXPECTED TO REPORT FOR JOB-SEARCH ASSISTANCE FOLLOW-UP ACTIVITIES	93
V.2	JOB-SEARCH ASSISTANCE FOLLOW-UP ACTIVITIES FOR THOSE EXPECTED TO REPORT: PERCENT WHO SATISFIED THE REPORTING REQUIREMENTS	95
V.3	JOB-SEARCH ASSISTANCE ACTIVITIES FOR THOSE EXPECTED TO REPORT: PERCENT WHO DID NOT SATISFY REPORTING REQUIREMENTS	97
V.4	DISTRIBUTION OF REASONS THAT NO FOLLOW-UP CALL-IN WAS NECESSARY	99
VI.1	TRAINING PARTICIPATION RATES FOR CLAIMANTS WHO WERE ASSESSED (Percent)	108
VI.2	TRAINING CHARACTERISTICS	110
VI.3	TRAINING OUTCOMES (Percent)	112

LIST OF TABLES (continued)

Tables Page

VI.4 DISTRIBUTION OF SERVICES PROVIDED TO JTPA TITLE III ENROLLEES, BY SDA, PROGRAM YEAR 85 117

VII.1 REEMPLOYMENT BONUS RECEIPT RATES, BY OFFICE FOR THOSE OFFERED THE BONUS (Percent) 128

VII.2 CHARACTERISTICS OF THE BONUS OFFER, APPLICATIONS, AND PAYMENTS 130

VIII.1 THE PROBABILITY OF A NONSEPARATION ISSUE, NONMONETARY DETERMINATION, OR DENIAL BY TREATMENT AND CONTROL GROUP STATUS (Percent) 138

VIII.2 LAST UI CLAIM DATE RELATIVE TO SCHEDULED ORIENTATION FOR CLAIMANTS NOT ATTENDING ORIENTATION (Percent) 140

VIII.3 LAST UI CLAIM DATE RELATIVE TO SCHEDULED JOB SEARCH WORKSHOP OR ASSESSMENT/ COUNSELING INTERVIEW FOR THOSE NOT ATTENDING (Percent) 141

X.1 ESTIMATED PER CLAIMANT EXPENDITURES FOR AN ONGOING PROGRAM (In Dollars) 163

X.2 ALTERNATIVE ESTIMATES OF PER CLAIMANT EXPENDITURES (In Dollars) 167

X.3 AVERAGE COST PER PARTICIPANT FOR JOB SEARCH ASSISTANCE SELECTED DISLOCATED WORKER DEMONSTRATIONS (Dollars) 169

FIGURES

Figure | | Page
IX.1 UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT--NEW JERSEY DEPARTMENT OF LABOR ORGANIZATION OF ADMINISTRATION--CENTRAL OFFICE PHASE II-IMPLEMENTATION 146

IX.2 UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT--ORGANIZATION OF LOCAL OFFICE PHASE II-IMPLEMENTATION 149

EXHIBITS

Exhibit		Page
III.1	NEW CLAIMANT QUESTIONNAIRE	49
III.2	NOTICE OF SELECTION FOR REEMPLOYMENT SERVICES	57
V.1	ES 572	92
VII.1	CLAIM FOR REEMPLOYMENT BONUS	125
VII.2	NEW JERSEY UI REEMPLOYMENT DEMONSTRATION PAYMENT VOUCHER	126
VIII.1	POLICY STATEMENT OF ADJUDICATION OF THE UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT	132
VIII.2	CLAIMANTS APPOINTMENT OR REFERRAL FOR SERVICES	134

I. INTRODUCTION

The Unemployment Insurance (UI) system provides short-term income support to involuntarily unemployed individuals while they seek work. The UI system also attempts to promote rapid reemployment by imposing various work-search requirements on UI claimants and by referring them to either the Employment Service (ES) and, through the ES, to services offered under the Job Training Partnership Act (JTPA). However, a number of observers have suggested that more intensive services should appropriately be given to UI claimants to help them become reemployed. It has further been argued that the more intensive reemployment assistance should be targeted toward permanently separated or displaced claimants who are expected to experience the greatest difficulty in becoming reemployed. It has also been argued that if reemployment assistance were provided early in the UI claim period the savings in UI benefit payments could potentially outweigh the costs of providing these services. In addition, even if paying for reemployment services for these workers does not prove cost-effective from the standpoint of UI, the UI system may play an important role by identifying a broad population of displaced workers early in their unemployment spells who could benefit from receiving the services.

The New Jersey Unemployment Insurance Reemployment Demonstration Project (NJUIRDP) was initiated by the U.S. Department of Labor (USDOL) through a cooperative agreement with the N.J. Department of Labor (NJDOL) to test whether the UI system can be used to identify displaced workers early in their unemployment spells and to test alternative, early intervention strategies to accelerate their return to work. Three packages of services, or treatments, were tested in the demonstration: (1) job search assistance only, (2) job search assistance combined with training or relocation assistance, and (3) job search assistance combined with a cash bonus for early reemployment. A key component of the demonstration was that eligible claimants were identified and services were provided through the coordinated efforts of the Unemployment Insurance (UI), Employment Service (ES), and Job Training Partnership Act (JTPA) systems.

The demonstration was initiated in July 1986, and, by the end of sample selection in June 1987, 8,675 UI claimants were offered one of the three service packages. Services to eligible claimants were continued into fall 1987 to ensure that all eligibles were able to receive, if desired, the full set of demonstration services.

The evaluation of the demonstration consists of two main components: (1) this implementation and process report, and (2) an impact and benefit-cost report (Corson et al., 1989). In addition, there is a summary paper (Corson, 1989) that presents the main findings of these two reports. The remainder of this chapter discusses the purpose of this implementation and process report (Section A); provides an overview of the demonstration design (Section B); briefly discusses the data used for this report (Section C); and outlines the remainder of the report (in Section D).

A. PURPOSE AND OBJECTIVES OF THE REPORT

Four major objectives underlie the implementation and process report. First, the report is intended to complement the impact analyses by assessing the importance of the various service components to the overall success or failure of each of the three service packages being tested in the demonstration. For example, if it is found that the job search assistance package was effective at reducing the length of time spent by claimants on UI, the purpose of the process analysis is to help judge which components of this treatment were most important in achieving this impact.

Second, the process analysis is intended to identify how the delivery of each service component could be improved or strengthened. For example, all the treatments included a one-week, half-day job-search workshop in which all types of claimants (e.g., blue-collar, white-collar) participated. The process analysis relies on observations obtained in site visits to examine whether it might have been better to provide separate workshops for different types of claimants or to have changed the duration of the workshop.

Third, by describing the demonstration services and the environment in which they were implemented, the process analysis helps assess whether the treatments can be replicated. For example, the New Jersey economy has been strong and dynamic during the demonstration implementation period. Although one cannot determine from the analysis exactly how the demonstration outcomes would have differed in a less robust economy, documenting the nature of the New Jersey economy and how it differed by local sites will help policymakers assess how the implementation of the demonstration treatments might differ in an alternative economic environment.

The fourth and final purpose of the process report is to determine the degree to which the demonstration was implemented as planned. This analysis, which is closely associated with the first purpose discussed above, will be helpful in interpreting the results of the impact analysis. An example will illustrate this point. Suppose that it is found that the process of collecting data on claimants to identify eligibles was faulty at one site. This finding might then lead to the decision to estimate the impacts of the demonstration by excluding this site, thus providing what is believed to be a more reliable estimate of impacts than could be obtained by using data from all the sites.

B. OVERVIEW OF THE DEMONSTRATION DESIGN

The NJUIRDP was designed to address three objectives: (1) to examine the extent to which UI claimants who could benefit from the provision of employment services can be identified early in their unemployment spells; (2) to assess the policies and adjustment strategies that are effective in helping such workers become reemployed, and (3) to examine how such a UI reemployment program should be implemented. To achieve these objectives, the design called for identifying demonstration-eligible individuals in the week following their first UI payment, and assigning eligible individuals randomly to three treatment groups that were offered alternative packages of reemployment services and to a control group that received existing services. The demonstration was implemented in 10 sites which corresponded to state UI offices. The sites were chosen randomly, with the probability of their selection proportional to the size of the UI population in each office.

1. Definition of Eligibility

The demonstration was intended to provide reemployment services to experienced workers who, having become unemployed through no fault of their own, were likely to face prolonged spells of unemployment. Their job-finding difficulties might be due to the unavailability of jobs, a mismatch between their skills and job requirements, or their lack of job-finding skills. However, because previous research efforts had failed to establish good predictors of prolonged unemployment spells (see, for example, Crosslin, Hanna, and Stevens, 1984), complex eligibility requirements could not be used to channel demonstration services.

Faced with this situation, the demonstration plan incorporated a small number of sample screens which were chosen to identify experienced workers who were likely to be displaced permanently from their jobs. Then the effects of the demonstration on subgroups of the eligible population were examined to determine if more complex screens would provide better targeting of demonstration services.

The following eligibility screens were chosen for the demonstration:

1. First Payment. The demonstration excluded claimants who did not receive a first UI payment. To promote early intervention, the demonstration also excluded claimants who did not receive a first payment within five weeks after the initial claim. Individuals who were working and, consequently, who received a partial first payment were also excluded, since their job attachment meant that they had not been displaced. Finally, claims of a "special" nature (e.g., Unemployment Compensation for ex-servicemembers, Unemployment Compensation for federal civilian employees, interstate claims, combined wage claims, etc.) were also excluded.

2. Age. An age screen was applied to eliminate the broad category of young workers who have traditionally shown limited attachment to the labor market and whose employment problems may be quite different from older, experienced workers. This

screen was set so that workers under 25 years of age were excluded from the demonstration.

3. Tenure. It was decided that demonstration-eligible claimants should have exhibited a substantial attachment to a job (or at least to have worked) so that the loss of a job was associated with one or more of the reemployment difficulties described above. This decision was implemented by requiring each claimant to have worked for his or her last employer for three years prior to applying for UI benefits and not to have worked full-time for any other employer during the three-year period. The three-year requirement is used by the Bureau of Labor Statistics to define dislocated workers (Flaim and Sehgal, 1985).

4. Temporary Layoffs. The demonstration treatments were not intended for workers who were facing only temporary layoffs. Thus, it was desirable that claimants on temporary layoff be excluded. However, previous research and experience show that many individuals expect to be recalled even when their chances of actual recall are slim. In order not to exclude such individuals from demonstration services, only individuals who both expected to be recalled and had a specific recall date were excluded.[1]

5. Union Hiring-Hall Arrangement. Individuals who are typically hired through union hiring halls exhibit a unique attachment to the labor market (as opposed to a specific job), and were thus excluded from the demonstration.

2. Treatments

As stated earlier, the demonstration tested three treatment packages for enhancing reemployment. Eligible claimants were assigned randomly to the three treatment groups (job-search assistance only, JSA plus training or relocation, and JSA plus a reemployment bonus) and to a control group which received services that were then currently available. The impact evaluation is based on a comparison of the alternative treatments with each other and with the current service environment (the control group) in order to measure the effect of the treatments on the claimants' employment, earnings, and UI receipt.

All the treatments began with a common set of initial components (notification, orientation, testing, a job-search workshop, and an assessment/counseling interview), which were delivered sequentially early in the claimants' unemployment spells. First, a notification letter was sent to claimants after they received their first payment, which was about the third week after the claim was filed; thus, claimants usually began to receive services during their fifth week of unemployment. At that time, they reported to a demonstration office (usually an ES office) and received orientation and testing during the same week. In the following week, they attended a week-long, half-day job-search workshop, and a follow-up, one-on-one counseling/assessment session scheduled for the following week. These initial treatment components were mandatory; failure to report could lead to the denial of UI benefits.

---

[1] A recent survey of UI claimants in ten states (Corson, Kerachsky and Kisker, 1987) found that about 40 percent of the claimants who expected to be recalled but did not have a definite recall date did not return to their former employer. Only 10 percent of those with a definite recall date did not return to their former employer. Most of those who did not expect to be recalled (89 percent) did not return to their former employer.

Beginning with the assessment/counseling interview, the nature of the three treatments differed. In the first treatment group--job-search assistance (JSA) only--claimants were told that as long as they continued to collect UI they were expected to maintain periodic contact with the demonstration office, either directly with staff to discuss their job-search activities or by engaging in activities at a resource center situated in the office. The resource center contained job-search materials and equipment such as job listings, telephones, and occupational and training literature. Claimants were encouraged to use the resource center actively, and were told that if they did not come to the office periodically they would be contacted by ES staff and asked to do so. These periodic follow-up contacts were to occur at 2, 4, 8, 12, and 16 weeks following the assessment interview.

Claimants in the second treatment group--JSA plus training or relocation--were also informed about the resource center and of their obligation to maintain contact during their job-search period. In addition, they were told about the availability of classroom and on-the-job training, and they were encouraged to pursue training if interested. Staff from the local JTPA Service Delivery Area (SDA) program operator worked directly with these claimants to pursue the training options. These claimants were also told about the availability of relocation assistance, which, if they elected not to pursue training, they could use to pay for out-of-area job search and for moving expenses.

Claimants in the third treatment group--JSA plus a reemployment bonus--were offered the same set of JSA services as was the first treatment group, but also a reemployment bonus. The maximum bonus equaled one-half of the claimant's remaining UI entitlement at the time of the assessment interview. This amount was available to the claimant if he or she started work either during the assessment week or in the next two weeks. Thereafter, the potential bonus declined at a rate of 10 percent of the original amount per week until it was no longer available. Claimants could not receive a bonus if they were recalled by their former employer, if the job was with a relative, or if the job was temporary, seasonal, or part-time. They received 60 percent of the bonus if they were employed for 4 weeks, and the remainder if they were employed for 12 weeks. This bonus was sizeable; the average initial bonus offer equalled $1,644. It was expected to provide a strong incentive to the claimant to engage in early, intensive job-search.

Each of these treatments tested a different view of the employment problems faced by displaced workers. More specifically, the JSA-only treatment was based on the assumption that displaced workers have marketable skills but do not have sufficient experience to identify these skills and sell them in the job market. In contrast, the training treatment was based on the assumption that the skills of the workers are outmoded in many cases and must be upgraded. Finally, the reemployment bonus treatment was based on the prmise that, while many displaced workers have marketable skills, they may lack the motivation to seek reemployment rapidly.

With the exception of the reemployment bonus and the relocation assistance, the services that were offered in the demonstration are similar to those that were available under the existing ES and JTPA systems in New Jersey. However, the likelihood that a claimant was offered and received these services in the demonstration was considerably greater than under the existing system. Moreover, the timing of service receipt also differed; demonstration services were generally provided earlier in the unemployment spell than were existing services.

3. Provision of Demonstration Services

An important objective of the demonstration was to examine how a reemployment program targeted toward UI claimants should be implemented. Two aspects of that objective were given considerable emphasis in the demonstration design phase: (1) using existing agencies and vendors to provide the services, and (2) using a computer-based participant tracking system to facilitate the delivery of services.

In the NJUIRDP, the first aspect meant that the UI agency, the ES, and the JTPA's local program operators were all involved in delivering services, and that strengthening linkages among these agencies was an important component of the demonstration. UI staff were responsible for collecting the data that were used to select eligible claimants, and for monitoring compliance by claimants with the demonstration's reporting requirements. A determination of UI eligibility was to be performed when claimants did not report for the initial mandatory services, and, if appropriate, benefits were to be denied.

The initial reemployment services, together with the additional services offered at the assessment/counseling interview, were provided in each demonstration office by a four-person team. This team consisted of three ES staff members--a counselor and two interviewers (one half-time)--and a three-quarter-time JTPA staff member from the local SDA program operator. The ES counselor was the team leader and had overall responsibility for the provision of services. ES staff provided all of the services for the JSA-only and JSA plus reemployment bonus treatment group members. The JTPA staff members were involved only with the JSA plus training/relocation treatment group members. They were expected to become involved with the claimants during the assessment/counseling interview and to work with individuals who were interested in classroom or on-the-job training to identify appropriate opportunities and to place the claimants in them. The goal was to use the training opportunities available in each local JTPA SDA. Thus, this component of the demonstration strengthened the linkages between the ES and the local JTPA program operators in the ten demonstration sites.

The other important aspect of the implementation was the extensive use of a computer-based tracking system to operate the program. Service delivery data were entered into the system, and local office staff were provided with weekly lists of claimants who were expected to receive services. A list of claimants who did not report for services was also generated for use by UI, and monitoring reports were provided to central office staff. The system helped ensure that services were delivered as specified, and that claimants were not "lost" from the program.

C. INFORMATION SOURCES

The observations made in this implementation and process report are based on five types of information collected during the demonstration. First, extensive data on the activities of individual claimants were collected. These data included data on claimants' participation in the various services offered by the demonstration which were recorded by local office staff in the computer-based Participant Tracking System (PTS). Most of the data on claimants' activities that are presented in this report came from this source although some data were also collected from UI, ES, and JTPA administrative records. In addition, a telephone interview was conducted with a subsample of claimants and information from this interview on claimants use and perceptions of the services are included in the report.

Second, periodic visits were made to each site, and information on the delivery of services was collected through direct observations of job-search workshops (for example) and through discussions with local office staff. A site visit protocol was used to ensure that comparable data were collected from each site.

Third, throughout the demonstration, periodic meetings were held by NJDOL with the staff from all offices. These meetings provided a forum for staff to discuss any operational problems and for central office staff to provide information to local office staff on new procedures or to reinforce old procedures in a way that was consistent for all offices. These meetings were attended by an evaluation staff member, and they provided useful information for this evaluation.

Fourth, discussions were held throughout the demonstration with NJDOL central office staff, focusing on their views about project operations; the information collected from these discussions has also been used in this report.

Finally, data on the administrative costs of the demonstration were obtained from the NJDOL accounting system and have been used extensively in the chapter on administrative costs.

D. OUTLINE OF THE REPORT

As indicated earlier, the purpose of this process report is to describe the implementation of the demonstration treatments and to assess the importance of each treatment component in achieving the measured impacts. This report complements the impact and benefit-cost report (Corson et al., 1989) which examines the impact of the demonstration on UI receipt and post program employment and earnings.

The remainder of this report is as follows. Chapter II describes the economic and institutional environment in each of the ten demonstration sites. The chapter also contains a brief description of the method used to select sites.

The next five chapters then examine the issues associated with the delivery of services to eligible claimants. Chapter III discusses the method used to identify and notify eligible claimants about the demonstration services. Each of the service packages began with a common set of services, and these initial services are discussed in Chapter IV. Chapters V, VI, and VII then discuss, respectively, the additional services offered to claimants in the three treatment groups: periodic job-search assistance, relocation and training, and the reemployment bonus. The participation of claimants and the experience of providing these services are examined.

Organizational and cost issues are addressed in the next three chapters. In Chapter VIII, the link between the UI system and the ES system, which attempted to monitor the compliance of claimants with the demonstration reporting requirements, is examined. Issues associated with the overall organizational and staffing arrangements are addressed in Chapter IX. Estimates of the administrative costs in providing the treatments are presented in Chapter X.

The final chapter discusses the issues associated with the replicability of the results and summarizes the findings of the implementation and process analysis.

II. THE LOCAL ENVIRONMENTS

In every demonstration effort, it is important that the environment within which the program operated be examined. In the NJUIRDP, it is important for three reasons. First, it helps us assess the generalizability of the results to the wider population of all displaced workers in New Jersey. Second, since the demonstration operated in and was influenced by local employment environments, we can gain further insight into how the demonstration was implemented. Third, it helps us interpret any differences among the sites in terms of the impacts of the program.

This chapter focuses on the sites that operated the NJUIRDP. It begins by reviewing the process by which local sites were chosen to participate in the demonstration. It then provides an overview of New Jersey's economy and describes the local employment environment in each site. Next, data on the demographic and economic composition of each local demonstration-eligible population are presented. The final section provides brief highlights of the local environments that comprised the demonstration.

A. SITE SELECTION[1]

An important evaluation objective of the demonstration was to rely on a research design that would enhance the validity and generalizability of the results as much as possible. An important element in achieving this objective was the random assignment of eligible claimants to the treatment and control groups. Equally important was choosing demonstration-eligible claimants in such a way that the results would be generalizable to the broader population of dislocated workers in New Jersey. Thus, underscoring the site selection process were three objectives:

1. To choose demonstration-eligible claimants from as broad a population of New Jersey's displaced workers as possible
2. To provide each potential eligible claimant with an equal probability of selection
3. To select a broad representation of types of local office settings (e.g., co-located ES and UI offices vs. those that are not co-located, and diverse training environments)

To achieve these objectives, 10 local offices were chosen from the 38 local UI offices as follows. First, 14 offices were excluded from the selection process because they were too small to support the demonstration. Three additional offices that served primarily seasonal workers or that were located in areas with very low rates of manufacturing employment were excluded because they were likely to exhibit low rates of worker dislocation. Second, local offices were stratified geographically to ensure that, as a group, the ten offices chosen were representative of the state in terms of industry, type of office setting, and other factors that may be associated with geographical location.

---

[1] A detailed discussion of site selection can be found in the NJUIRDP design document (Corson et al., 1986).

Finally, but most importantly, 10 local offices were randomly selected, with the probability of selection based on their size, as measured by the number of claimants who collected five or more weeks of benefits in FY 85. The following local offices were selected to participate in the demonstration:

- Paterson
- Hackensack
- Jersey City
- Butler
- Bloomfield
- Newark
- Elizabeth
- Perth Amboy
- Burlington
- Deptford

It should be noted that, as presented above, the sites are listed by geographical region, starting with the northeast portion of the state and continuing through to the southwest portion of the state. We use this ordering throughout the report.

B. THE CHARACTERISTICS OF THE SITES

During the operational phase of the demonstration, the economy of the state of New Jersey was quite strong and dynamic. While the United States as a whole had an unemployment rate of 7 percent during 1986, New Jersey's unemployment rate was lower, averaging 5 percent during that year. In addition, prior to and during the demonstration, a decline in manufacturing jobs in the state was accompanied by an expansion of nonmanufacturing employment, primarily in the service industries.

Table II.1 reflects these patterns for both the state and the local office areas, on average. While this general pattern is apparent in most of the individual sites, Table II.1 indicates that the nature and strength of the economies varied substantially among the sites.

TABLE II.1

EMPLOYMENT CHARACTERISTICS OF THE LABOR MARKET AREAS AND COUNTIES WITHIN WHICH DEMONSTRATION LOCAL OFFICES WERE LOCATED

	Bergen-Passaic Labor Market Area		Jersey City Labor Market Area	Newark Labor Market Area			Middlesex-Somerset-Hunterdon Labor Market Area	Camden Labor Market Area			
Characteristic	Paterson (Passaic Cty.)	Hackensack (Bergen Cty.)	Jersey City (Hudson Cty.)	Butler (Morris Cty.)	Bloomfield/Newark (Essex Cty.)	Elizabeth (Union Cty.)	Perth Amboy (Middlesex Cty.)	Burlington (Burlington Cty.)	Deptford (Gloucester Cty.)	All Sites	State of New Jersey
Population (1986 percent of state total)	6.1	11.0	7.3	5.5	11.1	6.6	8.4	5.0	2.8	63.8	7,625,000
Unemployment Rate (1986)	6.1	3.9	8.0	3.3	6.7	5.4	4.4	3.9	5.1	5.4	5.0
Percent of Covered Employment in (1986)											
Manufacturing	36.3	25.5	27.4	24.5	20.1	29.4	28.6	23.0	25.9	26.7	24.0
Nonmanufacturing	63.7	74.5	72.6	75.5	79.9	70.6	71.4	77.0	74.1	73.3	76.0
Wholesale and retail trade	26.2	34.6	29.0	24.1	24.5	24.0	30.2	33.0	37.1	29.3	29.0
Services	22.5	23.4	19.9	26.1	31.2	25.5	21.0	25.5	21.4	24.0	26.8
Other nonmanufacturing	15.0	16.5	23.7	25.3	24.2	21.1	20.2	18.5	15.6	20.1	20.2
Percent Change in Nonagricultural Employment (1977-1986)[a]											
Manufacturing	-3.4	-7.5	-27.2		-14.9		-4.7	13.7		-7.5	-9.7
Durable	9.9	-13.4	-49.4		-22.5		-6.8	19.3		-11.1	-12.4
Nondurable	-12.5	-2.4	-13.9		-6.8		-3.0	7.1		-4.5	-7.2
Non-manufacturing	20.7	30.4	15.6		26.0		53.2	38.1		30.0	35.0
Wholesale and retail trade	13.0	26.1	29.4		21.0		48.1	35.3		27.1	30.7
Services	54.0	51.5	35.2		50.1		86.4	62.0		55.2	63.0
Total	12.1	19.4	2.2		15.1		34.5	32.3		19.3	22.9

SOURCES: Data for all sites except Burlington and Deptford come from State of New Jersey, Department of Labor, Regional Labor Market Review: Northern New Jersey Region, August 1988. Data on Burlington and Deptford come from State of New Jersey, Department of Labor, Regional Labor Market Review: Southern New Jersey Region, May 1988. The percentage change in nonagricultural employment for the State of New Jersey was provided by New Jersey Department of Labor, Division of Planning and Research.

[a] Data for Paterson, Hackensack, and Jersey City are by county. All other data are presented for the labor market area within which the site is located.

The remainder of this section provides a more detailed description of each site, grouping them by labor market area.[2] We group the sites this way because some data are available only at this level of disaggregation. While other data are available and presented for smaller geographic units (primarily counties), it should be noted that the larger areas are probably more representative of the labor markets that face most claimants.[3] Indeed, given the suburban to urban nature of much of the state, over a third of the employed residents of each county within which the demonstration sites are located work outside of their county of residence. Thus, while the economic environment in each site is an important factor in explaining the employment outcomes of claimants, economic conditions in surrounding labor markets are also important given the geographic proximity of many of these labor markets, particularly in the northern part of the state.

1. Paterson and Hackensack (Part of the Bergen-Passaic Labor Market Area)

The Bergen-Passaic labor market area, in which the Paterson and Hackensack sites are located, is a suburban to urban area located in the northeast part of New Jersey. Paterson is one of three large cities in Passaic County. This county has traditionally depended on factory employment, and, as indicated in Table II.1, the percent of nonagricultural employment in manufacturing (36 percent) in Passaic County was high relative to both the other demonstration sites and the state as a whole. The county experienced a decline in manufacturing during the past 10 years due to foreign and domestic competition and to a fire in 1985 that destroyed about one-quarter of Passaic City's industrial employment base. During the period of the demonstration, the employment decline was concentrated in industries that produce nondurable goods, primarily chemicals, rubber, and plastic products. These cutbacks were offset somewhat by hiring in the apparel, electrical machinery, and fabricated metals industries. In addition, the nonmanufacturing sector (the service industries, in particular) grew substantially between 1977 and 1986, offsetting the losses of factories over this period.

The city of Paterson has traditionally been a manufacturing center (particularly in the apparel and textile industry). Paterson has reflected the county's trend in a reduction in manufacturing. In fact, during the demonstration, Paterson (and the other cities in the county) had a higher unemployment rate than the county as a whole (whose rate was higher than the state's). Like many other urban areas,

---

[2] Much of the descriptive information presented in Section C was obtained from the New Jersey Department of Labor's regional labor market reviews (NJDOL, 1987a, and NJDOL, 1987b).

[3] For some occupations, the relevant labor market is probably larger than the labor markets defined by the state.

Paterson has a high rate of poverty relative to its surrounding area. Indeed, Paterson's NJUIRDP office was located in a fairly depressed area, particularly compared with the other local offices in the demonstration.

Although Bergen County, in which Hackensack is located, is part of the same labor market area as Passaic County, its employment environment differs. The largest industry in Bergen County is trade (particularly wholesale trade). As in much of the state, growth in the trade and service industries helped expand the number of jobs in the county between 1977 and 1986. In addition, while manufacturing jobs decreased overall during that period, expansion did occur in the food and paper and allied products industries. This economic growth, accompanied by area development, a low unemployment rate (see Table II.1), and a high per capita income, reflected a strong economy during the demonstration. This description of Bergen County also provides a fairly accurate characterization of Hackensack, which has historically been a retail center.

2. Jersey City (Part of the Jersey City Labor Market Area)

Hudson County (in which Jersey City is located) is the most densely populated county in New Jersey. Given its location--on a peninsula between Newark and New York City--it is not surprising that transportation has traditionally been a prominent industry in the county. The apparel industry also provides a relatively large percentage of the area's manufacturing jobs.

Since the early 1980s, the county experienced growth in the nonmanufacturing sector, with the largest increases occurring in the construction industry. In contrast, the manufacturing industry in the county experienced a sharp decline. In the past ten years, manufacturing declined 27 percent, compared with an overall decline of 7 percent in the state. Since the early 1980s, the largest cutbacks have been in the durable goods industry.

Over the past ten years, the growth in nonmanufacturing jobs barely offset the decline in manufacturing, creating a stagnant economy. The county also had, for New Jersey, a relatively high rate of unemployment in 1986 (8 percent), which was the highest of all the demonstration sites. Hudson County can also be characterized as having a relatively older population and a population that shows a high rate of public assistance receipt.

3. Butler, Bloomfield, Newark, and Elizabeth (Part of the Newark Labor Market Area)

The Newark labor market area comprises four counties: Morris (in which Butler is located), Essex (in which Bloomfield and Newark are located), Union (in which Elizabeth is located), and Sussex.[4] As is the case throughout the state, this area has experienced growth in the nonmanufacturing sector and a decline in the manufacturing sector. The growth in jobs has occurred in the trade and service industries and in the finance/insurance/real estate and construction industries (particularly in Morris County and Essex County). In most cases, this growth has more than offset the substantial decline in manufacturing. For example, during the demonstration, Union County, the location of the Elizabeth Office, experienced steep reductions in manufacturing jobs in nearly every industry (with the largest declines occurring in the transportation equipment industry).

Despite exhibiting similar industrial trends, the counties in the Newark labor market area (and the cities included in the demonstration) represent quite different levels of urbanism and standards of living. The town of Butler, and Morris County as a whole, is a suburban to rural area (the county does have some agricultural industry) with a strong economy. As indicated in Table II.1, Morris County had the lowest rate of unemployment of any of the demonstration sites. (In fact, Butler's rate was even lower). In 1984, personal income per capita in Morris County was higher than the state average, and the rate of poverty in the county was the lowest of any county in the state in 1979. The town of Butler is located in the northern part of Morris County, close to the border of Passaic County. In fact, 66 percent of the treatment-group members in this office resided in Passaic County.

In contrast, Essex County, one of the most densely populated counties in the state, had a slower rate of economic growth during the last several years than the state as a whole. The unemployment rate in Essex County was above the state unemployment rate and higher than the rates of almost all the other counties in which the demonstration sites were located. This characterization is due primarily to the influence of the city of Newark, with its relatively high rate of unemployment (10.8 percent in 1986). In 1986, over half of the county's unemployed lived in Newark, which has the largest population of any city in New Jersey. Newark also has a high poverty rate, which, in 1979, was the highest rate of poverty in the nation.

In contrast to Newark, Bloomfield, which is also located in Essex County, is a less urban, higher income area. The population served by the demonstration office was primarily white-collar. The unemployment rate in Bloomfield itself was similar to the state unemployment rate during the demonstration period.

---

[4] Sussex County is a predominantly rural area, and did not contain any demonstration sites.

Like Essex County, Union County, in which Elizabeth is located, is also densely populated. Union County is somewhat unique in this labor market area, in that a relatively large percentage of its jobs are in manufacturing (as shown in Table II.1), particularly in the food, printing and publishing, and chemical industries. While economic growth in Union County has been slow, its unemployment rate was similar to the state unemployment rate in 1986, and its per capita income in 1984 was the fifth highest of all the counties in the state.

4. Perth Amboy (Part of the Middlesex-Somerset-Hunterdon Labor Market Area)

Middlesex County, in which Perth Amboy is located, is in the north-central portion of New Jersey. The largest industries in the county are trade and services. These industries have experienced substantial growth in the past 10 years, due primarily to an expansion in business and health services, shopping centers, and wholesale distribution facilities. While the largest manufacturing industry in the county is the chemical industry (with companies that produce flavors, fragrances, cosmetics, and pharmaceuticals), the largest declines in the manufacturing sector have also been in chemicals and allied products. Only two manufacturing industries have shown growth during the past ten years: printing/publishing and rubbers/plastics. A major manufacturing industry in Perth Amboy itself is steel fabrication.

The overall growth of the economy is reflected in a relatively low unemployment rate (see Table II.1). However, the unemployment rate in Perth Amboy (8.1 percent in 1986) was higher than the rate for Middlesex County, reflecting the less robust economy of Perth Amboy relative to the rest of the county. Perth Amboy is a small city, which is geographically isolated from the rest of the county. In the past, employment in Perth Amboy has been concentrated in the manufacturing sector; consequently, the shift from manufacturing to services has been felt in this city more than elsewhere in the county.

5. Burlington and Deptford (Part of the Camden Labor Market Area)

Burlington (in Burlington County) and Deptford (in Gloucester County) are located in a suburban to rural area in the southern portion of the state. The counties in which these sites are located (and Camden County, which is also part of the Camden labor market area) have experienced quite similar industrial and labor market trends.

In both counties, trade constitutes a relatively high proportion of nonagricultural employment (see Table II.1). During the past ten years (and during the demonstration), substantial growth in nonmanufacturing occurred in these counties, particularly in retail trade (due to the growth of suburban shopping centers and retail outlets) and services. Unlike state trends, this area has also experienced growth, though less substantial, in manufacturing employment. Although Gloucester County has traditionally been known for its petroleum-refining and chemical-processing industries, the highest concentration of manufacturing jobs in both Burlington and Gloucester Counties has more recently been in the electrical machinery industry. A number of large chemical companies have located in Burlington in recent years.

The unemployment rates for these counties, presented in Table II.1, support this characterization. The unemployment rate in Gloucester County in 1986 was similar to the state unemployment rate, while the rate in Burlington County was 1.1 percentage points lower, making it, together with Butler and Hackensack, one of the three sites in the demonstration that has a strong local economy.

C. CHARACTERISTICS OF THE ELIGIBLE POPULATION

Table II.2 presents the characteristics of the demonstration-eligible population by site. As indicated in the table, the sites were generally similar in terms of the sex and age distribution of eligible claimants, although a few small differences do stand out. Burlington had a larger percentage (58 percent) of eligible males than the average (52 percent). Deptford had a somewhat younger population than average, and Bloomfield a somewhat older one.

The ethnic composition of the sites varied substantially. Some sites (Butler, Deptford, Bloomfield, and Burlington) had a predominantly white eligible population, while the primary ethnic group in Paterson and Jersey City was Hispanic. Newark had a predominantly black or Hispanic claimant population.

The data on the industry of the base-period employer largely reflect the primary industries in each area, as discussed earlier. In particular, the claimants in the Paterson, Jersey City, and Newark sites were more likely to have worked in manufacturing than were claimants in the other sites. The major single industry for claimants in these sites was the apparel industry.[5] Claimants in these sites tended to have lower base period earnings, a lower average number of base period weeks worked,

---

[5] The proportion of eligible claimants who had worked in the apparel industry was 17 percent in Paterson, 27 percent in Jersey City, and 22 percent in Newark.

TABLE II.2
CHARACTERISTICS OF THE NJUIRDP ELIGIBLE POPULATION, BY OFFICE
(percent)

	Office										
	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
DEMOGRAPHIC VARIABLES											
Sex											
Male	50.3	48.2	54.4	50.9	48.1	50.0	53.6	55.4	57.6	53.2	52.1
Female	49.7	51.8	45.6	49.1	51.9	50.0	46.4	44.6	42.4	46.8	47.9
Age											
25-34	23.4	24.6	22.1	30.2	23.7	24.5	25.1	25.6	24.5	37.2	25.8
35-44	29.8	24.5	28.9	25.2	23.2	28.8	23.4	26.0	26.4	26.7	26.3
45-54	24.4	21.7	27.1	20.7	19.2	24.2	24.6	22.0	24.6	18.4	22.8
55-64	16.4	21.6	17.0	19.4	24.0	18.7	21.8	22.5	19.6	15.2	19.8
65 or older	6.0	7.6	5.0	4.6	9.9	3.7	5.1	3.9	4.8	2.6	5.3
Mean	44.1	45.4	44.4	43.3	46.4	43.7	44.9	44.1	44.2	41.0	44.2
Ethnic Group											
White	24.9	76.4	27.9	99.6	86.6	18.0	53.7	72.9	83.2	90.2	60.9
Black	25.5	8.5	27.6	0.1	9.8	45.8	16.3	5.6	13.8	8.4	17.2
Hispanic	48.4	11.0	35.0	0.0	1.7	35.2	27.9	20.4	1.3	0.9	19.5
Other	1.2	4.1	9.5	0.3	1.9	10.0	2.1	1.1	1.7	0.5	2.4
BASE PERIOD EMPLOYMENT											
Industry of Base Period Employer[a]											
Manufacturing	66.6	41.7	51.6	35.6	36.7	48.1	47.7	47.4	38.5	41.3	47.2
Durable goods	27.6	19.6	12.1	20.5	22.8	18.9	24.1	23.1	26.6	27.1	23.4
Nondurable goods	39.0	22.1	39.5	15.1	13.9	29.2	23.6	24.3	11.9	14.2	23.8
Nonmanufacturing[b]	33.4	58.3	48.4	64.4	63.3	51.9	52.3	52.6	61.5	58.7	52.8
Mean Earnings	$13,500	$19,800	$13,500	$22,900	$21,600	$13,500	$18,800	$20,300	$20,700	$17,900	$18,000
Mean Number of Weeks Worked	43.2	46.6	41.7	48.2	47.3	43.2	45.4	46.1	46.5	45.7	45.3
Expected Recall[c]	57.5	25.3	54.6	19.2	23.6	54.5	31.2	27.6	30.2	31.1	36.2
UI ENTITLEMENT											
Mean Weekly Benefit Rate	$162	$189	$167	$196	$192	$164	$186	$188	$185	$183	$181
Mean Entitlement	$4,030	$4,774	$4,063	$5,007	$4,888	$4,048	$4,681	$4,740	$4,678	$4,596	$4,534
Mean Potential Duration	24.7	25.1	24.2	25.5	25.2	24.6	25.1	25.1	25.2	25.0	25.0
Sample Size	1,041	1,171	1,084	743	1,118	1,337	1,383	1,198	1,003	982	11,060

NOTE: The percentage distributions are reported except in the dollar figure columns.

[a] The industry of the employer with the largest earnings listed in the UI database is reported when there is more than one employer.

[b] Includes cases where information was not available.

[c] The percentage expecting recall is the percentage who said that they expected recall in the new claimant questionnaire but who were eligible because they did not have a recall date.

lower UI weekly benefit rates and entitlements, and a shorter potential duration of benefits. A much higher percentage of claimants at these offices also expected to be recalled but had no definite recall date. Both their lower earnings and benefits and their recall expectations are probably due to the relatively high proportion of eligible claimants in the apparel industry, an industry which is characterized by low wages and seasonal fluctuations.

D. SUMMARY

In summary, during the demonstration, New Jersey's economy was quite strong and growing, with the decline in manufacturing employment offset by an expansion in nonmanufacturing employment. While these trends were apparent in each of the demonstration sites, the nature and strength of the economies of the sites varied substantially. In addition, the characteristics of the demonstration-eligible population differed noticeably among the sites.

In particular, four differences among the sites are worth noting:

1. Hackensack, Butler, and Burlington had particularly strong economies.
2. Paterson, Jersey City, Newark, and Perth Amboy had much weaker economies.
3. The primary ethnic group in Paterson and Jersey City was Hispanic, and Newark's claimant population was predominantly black or Hispanic.
4. Demonstration-eligible claimants in the Paterson, Jersey City, and Newark sites had lower base period earnings, a lower average number of weeks worked, and lower UI benefits and duration of benefits, and were more likely to expect to be recalled. These characteristics appear to reflect, at least in part, the influence of the apparel industry at those sites.

As we shall see in later chapters, these observations are helpful in analyzing the demonstration experiences of the sites.

III. IDENTIFYING AND NOTIFYING THE ELIGIBLE POPULATION

The first step in the delivery of demonstration services was to identify eligible claimants and notify them about their eligibility. This process occurred during the first several weeks of the UI claims process. It entailed collecting screening data on all claimants, processing these data to determine which ones met the demonstration eligibility criteria, assigning eligible claimants to the treatment and control groups, and sending letters to the claimants to ask them to report for services.

In general, the specific manner in which this process was accomplished would not be followed in an ongoing program, although the overall functions would. For example, not all the screening data used in the demonstration were routinely collected and data-entered by the UI system, which necessitated an additional data collection step for the demonstration. In an ongoing program, these data items would presumably be added to the state's UI data processing system. Similarly, most of the data processing was performed on a stand-alone microcomputer, rather than on the state's mainframe--a situation which is likely to differ in an ongoing program. For these reasons, much of the discussion in this chapter is probably not of direct use to future program designers. However, it is useful to the designers of future special programs or demonstrations and to our assessment of the validity of the NJUIRDP results.

Our discussion of these issues consists of four sections. In Section A, we describe and examine the process used to apply these eligibility criteria. Then in Section B, we describe how claimants were assigned to the treatment packages, and how they were notified about their selection. Section C examines the importance of the individual eligibility criteria. And Section D briefly compares the characteristics of the eligible population with the characteristics of a sample of noneligibles selected from the demonstration offices.

A. IDENTIFYING THE ELIGIBLE POPULATION

The demonstration applied seven specific eligibility screens to claimants who received a first UI payment under the regular state UI program (these screens are described in more detail in Chapter I). These screens excluded claimants who (1) were younger than age 25; (2) had a gap between the date of their claim filing and their first payment of 5 weeks or more; (3) were receiving partial payments because of earnings; (4) had not worked with their pre-UI employer three years before applying for UI; (5) had worked full-time for more than one employer during this three-year period; (6) were on temporary layoff and had a definite recall date; or (7) used an approved union hiring hall to secure employment.

These eligibility screens were applied through a weekly, six-step process. In the first step, a computer file was constructed to identify all UI claimants who received a first payment during the week. This file was constructed on the mainframe computer system used by the NJUI program. Then, in the second step, several screening criteria were applied to produce a file that contained a subset of the claimants who received first payments. The criteria that were applied at this point were based on data that are routinely collected by the UI system (such as age). In the third step, the file with this subset of claimants was downloaded to a MicroVax computer, which contained the demonstration's Participant Tracking System (PTS). In the fourth step, the downloaded files were matched with files that contained additional information on claimants that was used to identify eligible claimants. This additional information was collected for all claimants on a "New Claimant Questionnaire" (NCQ) (see Exhibit III.1) and was data-entered into the tracking system. The NCQ was a form designed to collect data for demonstration screening, and is not otherwise collected by local UI offices. Claimants filled it out at the time of the Benefits Rights Interview (BRI). In the fifth step, the additional information was used to identify eligible claimants. Finally, eligible claimants were assigned randomly to the treatment or control groups. This process was performed weekly during the year-long demonstration intake period (July 1986 to June 1987).[1]

There were several places in this process where potentially eligible claimants might have become lost from the sample frame. If this occurred in a systematic way, it is possible that the population of demonstration-eligible claimants who were actually selected for the demonstration might have differed from the "true" set of eligibles in ways that might have affected the results of the demonstration. To investigate this possibility, we examine three ways in which eligible claimants might have become lost from the system.

First, it is possible that all first payments might not have been captured in the first step, or that the screening criteria applied in the NJ mainframe might have been incorrect in some way. This potential problem was assessed both by examining the records of individuals who were selected and those who were not to determine whether the sampling criteria were applied correctly and

---

[1] Fifty weekly samples were selected during the one-year period. One week was skipped, by design, during the winter holiday period, and one week was skipped in February because of hardware problems.

Exhibit III.1

NEW CLAIMANT QUESTIONNAIRE
(please print)

Social Security Number: ____________________

B.R.I. Date: ____________________ Date of Birth: ____________________
(Month) (Day) (Year)

Name: ____
(Last) (First) (Middle Initial)

Please answer each question by placing an "x" in the numbered box.

1. Do you customarily secure work through a union?

☐ No 2 ☐ Yes 1 Which one? ____________________

2. Have you worked for the same employer for the past three years - - - mostly full time?

☐ No 2 ☐ Yes 1

2a. During the past three years, did you also work full time for someone other than the employer who just laid you off? (Full time is 32 hours or more per week for one month or longer.)

☐ No 2 ☐ Yes 1

3. Do you expect to be recalled by the employer who just laid you off?

☐ No 2 ☐ Yes 1

3a. Do you have a definite recall date from the employer who just laid you off?

☐ No 2 ☐ Yes 1

If so, when ____________________
(Month) (Day)

FOR OFFICE USE ONLY: Is the union specified on Item 1 on the list of unions certified as an approved hiring hall?

☐ No 2 ☐ Yes 1

LOCAL OFFICE CODE: ☐☐☐☐ DATE ENTERED ____________________ by comparing on an ongoing basis the number of first payments made each week with the number downloaded to the MicroVax. After several initial adjustments, there was no evidence that any problem existed in this process.

Second, it is possible that the screening criteria based on data from the New Claimant Questionnaire might have been applied incorrectly, or that the data themselves might have been incorrect or incorrectly data-entered. Records were again examined to explore this first possible problem, and no problems were found. The data entry of the questionnaire was also carefully controlled through an extensive set of edit checks. However, when data were missing, questionnaires were returned to UI. Some may not have been returned in a timely enough manner for sampling, leading to some leakage from the sample. Some evidence also came to light early in the demonstration that the questions on job tenure and recall status may have been incorrectly answered by some claimants. However, the direction of both errors was to include "ineligible" claimants rather than to exclude "eligible" claimants. The instructions that accompanied the questionnaire were adjusted in response to these problems, although it is still likely that some claimants were still incorrectly assigned to the demonstration.

The final area in which sampling problems could have occurred pertains to the matching of New Claimant Questionnaires with downloaded data. All downloaded records, except those filed as part of a mass temporary layoff, should have had matching questionnaires. However, as reported in Table III.1, 85 percent of all downloaded cases were ultimately matched to questionnaires, leaving 15 percent unmatched. Moreover, an analysis of the extent of mass temporary layoff claims suggests that they accounted for, at most, one-eighth of the unmatched cases.[2] Thus, a large enough percentage of unexplained unmatched cases exists to suggest that sampling might have been biased; however, we have no reason to believe that the occurrence of cases which should have had but did not have questionnaires was systematic.

This issue of unmatched cases was of concern to demonstration staff, and two steps were taken early in the demonstration to improve the extent to which matching occurred. Throughout the demonstration, New Claimant Questionnaires were completed by claimants during their Benefit Rights Interview (BRI), which in New Jersey takes place somewhere between the initial claim and the

---

[2] Claimants who filed for UI as part of a mass temporary layoff did not fill out questionnaires. Although they were ineligible for the demonstration, their files could have been downloaded, since at the time the project was developed there was no way to identify such claims on the N.J. mainframe. A subsequent change in the system was made to permit identifying these claims, and we found that, at most, 2 percent of the downloaded cases were part of a mass temporary layoff.

TABLE III.1

MATCHING OF FIRST PAYMENTS WITH NEW CLAIMANT QUESTIONNAIRE DATA, BY QUARTER OF SAMPLING

Quarter[a]	Number of Records Downloaded	Percent Matched with New Claimant Questionnaires	
		Initially	Ever
1986.3[b]	10,789	66	82
1986.4	9,548	86	89
1987.1[c]	13,667	66	82
1987.2	9,993	76	88
Total	43,997	74	85

[a] The quarter refers to the quarter in which sampling occurred.

[b] One week in August is excluded from this calculation because not all data were available.

[c] Two weeks in which hardware problems affected the percent matched are excluded from the calculation (one in January and one in February).

first payment date, depending on the local office and the volume of initial claims. Initially, the questionnaires were collected and filed with each claim. They were then pulled when and if a first payment was made and were sent to the Employment Service for data entry. The primary purpose of this process was to reduce data entry time, since not all individuals who file a new initial claim receive a first payment.[3] Because this process was believed to be contributing to the leakage of claimants from the demonstration sampling process, beginning in mid-September all New Claimant Questionnaires were sent directly from the BRI to the ES local office for data entry, without waiting for the first payment to be determined. The clerks who administered the BRIs were also given further instructions about the importance of collecting a New Claimant Questionnaire from everyone and the importance of checking questionnaires for completeness. As indicated in Table III.1, these changes may have improved the matching process, since the percentage matched rose substantially between the first and second quarter of sampling. However, this percentage declined in the third quarter of sampling before again rising in the final quarter.

The data in Table III.1 also indicate that about 74 percent of the downloaded records were initially matched. When cases for a particular week were downloaded, they were matched to questionnaires and then sampled. This was the initial match. If no match was found, the cases were maintained in a pending file; if a match occurred within the next two weeks (i.e., when the questionnaire was later data-entered), the case was included in the sample. The percentage of cases initially matched was low in both the first and third quarters of sampling. The low initial level was probably caused by the matching procedures used initially in the demonstration (see the discussion in the preceding paragraph), while the low level of initial matching in the 1987.1 period was probably due to the relatively large volume of claims handled in that period. Substantial data-entry backlogs were encountered in some offices during this period, contributing to both a slippage in the sampling process and probably the overall low level of matching in this period. The relatively low levels of initial matches recorded in the first and third quarters of sampling mean that service delivery probably lagged more in these periods than in the other quarters.

The data in Table III.2 indicate that, similar to Table III.1, local offices whose volume of downloaded cases was greater generally had a lower initial-match percentage and a lower ever-

---

[3] During the July 1986 to June 1987 period, the number of first payments (in the demonstration offices) equalled 72 percent of the number of new initial claims.

TABLE III.2

MATCHING OF FIRST PAYMENTS WITH NEW CLAIMANT QUESTIONNAIRE DATA, BY LOCAL OFFICE

Local Office	Number of Records Downloaded	Percent Matched with New Claimant Questionnaires	
		Initially	Ever
Paterson	3,520	61	78
Hackensack	3,102	86	91
Jersey City	3,689	63	82
Butler	2,070	89	96
Bloomfield	2,713	84	94
Newark	6,105	52	86
Elizabeth	5,410	86	95
Perth Amboy	4,185	74	83
Burlington	3,132	88	92
Deptford	3,285	80	85

NOTE: The table excludes data for July and August because data were unavailable by office and two weeks (one each in January and February) in which hardware problems affected the percent matched.

matched percentage than offices whose volume of cases was smaller. The notable exception to this pattern is the local office in Elizabeth, which experienced large numbers of downloaded cases, but maintained a high level of both the percentage initially matched and the percentage ever matched. The percentage of cases initially matched differed considerably among the offices; however, the range of percentage ever matched among offices was much smaller. Newark, which had the largest volume of claims and hence the largest data entry load, had the lowest initial match percentage at 52 percent, while Butler had both the highest initial match percentage (89 percent) and the highest ever matched (96 percent). Paterson had the lowest percentage ever matched (78 percent). Butler also had the least number of downloaded records.[4]

B. ASSIGNMENT TO AND NOTIFICATION OF TREATMENT STATUS

The design of the NJUIRDP called for the random assignment of eligible claimants to one of the three treatment groups or to the control group. As described earlier, claimants were randomly assigned by first identifying all eligible claimants for whom data were available. The records for eligible claimants were then placed in random order and the assignments made according to a fixed schedule that assigned the first claimant to one treatment, the second to another treatment, and so on up to a fixed number per site per week. A higher proportion of claimants were assigned to treatment 2--job search assistance plus training and relocation--than to any other group because of special policy interest in the subset of the group who actually participated in training. Initially, up to 18 treatments and 5 controls were assigned per site per week. This number was increased to 26 treatments and 7 controls after the sites gained experience with delivering the demonstration services (beginning in September 1986). A further adjustment was made at the end of November 1986 to reduce random selection to a maximum of 23 treatments and 6 controls when it became clear that the initial increase in assignments provided more claimants per week than the sites could accommodate with available space and staff. This maximum was maintained for the remainder of the project. As shown in Table III.3, a total of 11,060 claimants were selected for the project.

Once the eligible claimants were identified and assigned to the treatment and control groups, a letter was produced and mailed to each claimant assigned to a treatment group. This letter (see

---

[4] The low level of downloaded records in Butler occurred because this office had the lowest number of UI claimants of any demonstration office and because this office was closed one month prior to the end of the sampling period.

TABLE III.3

DISTRIBUTION OF NJUIRDP ELIGIBLE CLAIMANTS BY TREATMENT, CONTROL STATUS AND OFFICE

	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
Job-Search Assistance Only (JSA)	229	257	236	160	247	291	300	260	218	218	2,416
JSA Plus Training and Relocation	359	405	375	252	381	466	478	412	346	336	3,810
JSA Plus Reemployment Bonus	228	257	242	169	249	292	303	267	225	217	2,449
Controls	225	252	231	162	241	288	302	259	214	211	2,385
Total	1,041	1,171	1,084	743	1,118	1,337	1,383	1,1[illegible]	1,003	982	11,060

Exhibit III.2) was signed by the state UI director, and informed the claimant to report for the demonstration orientation session. The letter included the claimant's name and address, and the date, time, and location of the appropriate local office orientation session.[5] The letter also told the claimant that failure to report could affect his or her eligibility for unemployment benefits.

The entire process of eligibility determination, treatment assignment, and notification was carried out on a weekly cycle. First, on the Monday following the first payment week, a file with potentially eligible claimants was downloaded to the MicroVax, after the initial mainframe screening process was undertaken (described above). The sample was then selected on Tuesday, and the notification letters were mailed on Tuesday or Wednesday, depending on how long sample selection took. Claimants were told to report for the orientation sessions to be held the following week. Since most claimants received their first payment in the third week of their claim, sample selection generally took place during the fourth week and the orientation session during the fifth week of the claims process. As discussed earlier, New Claimant Questionnaires were not always entered in a timely manner. In this case, sample selection occurred during either the fifth or sixth week of the claim, and orientation occurred the following week, since downloaded files without questionnaires were maintained in pending status for two weeks. If a match occurred during either of those two weeks, the claimant was included in the sample frame. The timing of service receipt is discussed further in the next chapter.

C. IMPORTANCE OF THE FACTORS THAT AFFECT ELIGIBILITY

Data on the impact of the eligibility screens described earlier are reported in Table III.4 by local office. The data in the table show the percentage of first payments under the regular state program that were excluded by the various eligibility screens.[6] The combined effect of all the screens is also reported. This combined effect is not the sum of the individual effects, since an individual may have been excluded for more than one reason.

The first panel in the table shows the impact of the three screens that were applied on the mainframe. As can be seen, the three mainframe screens together excluded 28 percent of the claimants who received a first payment. The age screen (15 percent) and the payment-timing screen (14 percent) were the most important. This latter eligibility screen was used to exclude claimants

---

[5] These sessions were held at the same time each week in each location.

[6] Because these data are for first payments under the regular state program, the effect of excluding claimants from special programs (UCFE, UCX) is not shown.

EXHIBIT III.2

STATE OF NEW JERSEY
DEPARTMENT OF LABOR
DIVISION OF UNEMPLOYMENT AND DISABILITY INSURANCE
LABOR AND INDUSTRY BUILDING
TRENTON, NEW JERSEY 08625 - 0058

CHARLES SERRAINO
Commissioner

JAMES A. WARE
Director

Notice of Selection for Reemployment Services

You have been selected to participate in the Unemployment Insurance Reemployment Demonstration Project which was briefly explained to you during your Benefit Rights Interview.

You are hereby directed to report to:

The project services will be explained to you when you report. Please be prepared to spend up to four hours receiving an orientation and other employment and training services. Failure to report may affect your eligibility for unemployment benefits. This appointment will take the place of any other appointment you currently have to register for work with the Employment Service.

If this appointment conflicts with either your regular reporting date for your benefit check or any other scheduled appointment with the unemployment claims office, please contact the UI Coordinator at the phone number listed above. Do not report if you are working.

Please bring this letter with you when you report for orientation.

Sincerely,

James A. Ware

James A. Ware
Director

New Jersey Is An Equal Opportunity Employer

AD-18.11 (9-84)

TABLE III.4

IMPACT OF THE ELIGIBILITY SCREENS ON FIRST PAYMENTS
UNDER THE REGULAR STATE UI PROGRAM, BY LOCAL OFFICE

	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
MAINFRAME SCREENS											
Percent Excluded by Age Screen	17.1	10.2	18.4	12.9	13.1	16.7	12.7	13.0	13.9	17.6	14.8
Percent Excluded by the Payment Timing Screen	18.9	8.4	16.2	12.1	8.7	17.7	14.9	12.7	10.4	14.4	14.1
Percent Excluded by the Earnings Screen	4.4	6.2	2.5	2.7	7.4	2.0	2.9	3.2	6.3	6.2	4.0
Percent Excluded by Mainframe Screens	33.6	21.5	28.5	23.1	25.7	31.6	26.8	25.9	24.6	32.0	27.9
MICROVAX SCREENS											
Percent Excluded by the Tenure Screen	43.6	47.8	43.0	51.2	48.2	45.3	47.8	45.2	53.9	51.9	47.5
Percent Excluded by the Single Employer Screen	6.3	3.4	6.1	1.8	3.7	6.7	2.8	4.4	3.4	4.0	4.4
Percent Excluded by the Temporary Layoff Screen	14.1	11.9	8.8	21.3	11.4	6.4	16.8	16.6	13.6	15.7	13.3
Percent Excluded by the Union Screen	5.4	7.7	9.2	14.5	7.1	9.1	10.2	13.0	10.4	16.5	10.2
Percent Excluded by Micro-Vax Screens	61.2	61.3	58.9	63.4	61.8	59.4	62.9	64.2	67.7	68.8	63.1
PERCENT EXCLUDED BY ALL SCREENS	74.2	69.6	70.6	71.9	71.6	72.2	72.8	73.5	75.6	78.8	73.4
Number with Matched Records on Tracking System	3,191	3,430	3,425	2,491	3,187	5,913	6,076	4,235	3,280	3,374	38,602

NOTE: The first set of screens (age, payment timing, and earnings) was applied on the state's mainframe computer. The estimated effects of the screens are based on tabulations performed by NJDOL following the end of sample selection. A file was created of all first payments in the regular UI program in the 10 demonstration offices over the year of sample selection. This file contained 75,120 records. The sample selection criteria applied on the mainframe were then applied to this file to provide an estimate of the percentage of noneligibles, which was 27.9 percent. A sample of noneligibles was drawn from this file and used to estimate the effect of the individual mainframe screens. The MicroVax screens were applied to the records downloaded from the mainframe (i.e., to the 72.1 percent of cases that passed the mainframe screens) that were matched with tracking system New Claimant Questionnaire data. Thus, the reported effect of these screens is their effect on the subset of first payments that passed the mainframe screens.

whose gap between their initial claim and their first payment was more than 5 weeks, and was applied because one of the primary objectives of the demonstration was to offer services early in the claim spell. However, because claimants who experience a delay in receiving a first payment tend to be those for whom an eligibility issue is raised about the reason for their job separation, it had the effect of excluding such claimants.

The remainder of the table shows the impact of the eligibility screens that were applied on the Microvax to the records that were downloaded from the mainframe.[7] Of the four screens that were applied at that point, the tenure screen was by far the most important. This screen excluded individuals who had not worked for their pre-UI employer three years previously, and it excluded almost half of the potentially eligible claimants who passed the mainframe screens.

Another important screen was the one which excluded claimants who had a definite recall date. As shown in the table, about 13 percent of the downloaded population were excluded by this screen. Although not shown in the table, the importance of this screen varied considerably by month, having been most important in July and August 1986 and January and February 1987. In devising this screen, it was decided that some evidence that the layoff was indeed temporary had to be established, rather than relying merely on the claimant's expectation that it was temporary. Having a definite recall date was used for this purpose. However, the claimant questionnaire also asked the more general question about recall expectations. As expected, a substantially larger percentage of claimants said that their layoff was temporary (44 percent) than said that they had a definite recall date (13 percent). In the analysis, the impact of recall expectations on program participation and on the impact of the demonstration treatments is examined.

A third screen which was also important was the one which excluded individuals who rely on a certified union hiring hall to find jobs. The impact of this screen also varied substantially over the year, having been most important in the January to March 1987 period (the maximum percentage excluded by the screen was 23 percent in February). Overall, 10 percent of the downloaded claimants were excluded by this screen.

---

[7] Although these screens were applied only to the downloaded cases, it is likely that, if all the screens were applied to the full population of first payments, the relative importance of each screen would be similar to that observed for the downloaded cases, although the percentage excluded by each screen would differ somewhat. In particular, the tenure screen would probably exclude a smaller percentage of the full population than was the case for the downloaded cases.

The fourth screen applied on the MicroVax--which excluded individuals who, while working for one employer for the past three years, also worked full-time for another employer--had relatively little impact on eligibility. About 4 percent of downloaded claimants were excluded by the single employer requirement.

Table III.4 also shows the impact of the eligibility screens by local office. While all screens varied somewhat by local office, the one which varied the most was the temporary layoff screen. The percent excluded by this screen varied from a low of 6 percent in Newark to a high of 22 percent in Butler. This variation primarily reflects differences in the industrial base in the various sites. In fact, as mentioned in Chapter II, Newark and Jersey City (a site in which a low percentage were excluded by this screen) were two of the sites in which a large percentage of claimants expected to be recalled but had no definite recall date. These claimants were primarily in the apparel industry.

The certified union hiring exclusion screen also showed a great deal of variability among local offices. Paterson had the smallest percent excluded (5 percent), while Deptford had the highest percent excluded (17 percent).

Despite the variation in the impact of individual screens by office, the overall percentages who were excluded varied less among offices. Hackensack had the lowest percent excluded (70 percent), and Deptford the highest (79 percent).

D. THE CHARACTERISTICS OF THE ELIGIBLE POPULATION

The purpose of applying the eligibility screens used in the NJUIRDP was to focus the offer of demonstration services on claimants who, in the absence of services, were expected to experience difficulty in becoming reemployed. Therefore, these claimants were also those who were expected to be long-term recipients of UI benefits. However, since previous research indicated that it was difficult to predict prolonged unemployment spells, some uncertainty existed about whether the eligibility screens chosen in the demonstration would achieve the objective of directing services to the long-term unemployed. This section examines this question by comparing the pre-UI characteristics and subsequent labor-market experiences of demonstration eligibles with those of a sample of noneligibles.[8] We show that the eligibility screens applied in the demonstration appear to have directed services successfully to the long-term unemployed.

---

[8] This sample was selected from among claimants who received a first payment under the regular state UI program from one of the demonstration offices during the demonstration intake period. For more detail on how the sample was selected see Chapter II in the impact and benefit-cost report.

The data reported in Table III.5 show the characteristics of eligibles and noneligibles prior to their UI receipt. The major statistically significant differences between the two groups were as follows:[9]

- Noneligibles were younger than eligibles on average.
- Noneligibles were more likely than eligibles to be males.
- Eligibles were more likely than noneligibles to be in manufacturing and less likely to be in construction.
- Base period and pre-UI weekly wages were higher for eligibles than for noneligibles, as were weeks worked in the base period. Because of these differences, UI entitlements and the weekly benefit rate was also higher for eligibles than for noneligibles.
- Eligibles were more likely than noneligibles to have worked for three years or longer at their pre-UI job.[10]
- Eligibles were more likely than noneligibles to have been laid-off, and it appears that these layoffs were more likely to be permanent, since a higher proportion occurred because the plant or facility closed, the company moved, or a shift was eliminated.

These differences can generally be related to the eligibility screens. For example, the age difference arose in part because individuals younger than 25 years of age were not included in the demonstration, and in part for other reasons, such as the focus of the demonstration on individuals who had been employed with the same employer for three years (if one excludes the younger than age 25 group from the comparison, the noneligibles were still younger than eligibles). Similarly, the industry, earnings, and job separation reasons appear to arise from the attempt to focus on permanently displaced, experienced workers.

The characteristics of the NJUIRDP-eligible population can also be compared with the characteristics of the general displaced or dislocated population identified by the Bureau of Labor Statistics (Flaim and Sehgal, 1985).[11] This population comprised a greater percentage of males than did the demonstration-eligible population (65 percent versus 52 percent). But along other dimensions

---

[9] Unless otherwise noted, the term "statistically significant differences" is used in this report for differences that are significant at the 95 percent confidence level for a two-tail test.

[10] Although the purpose of the eligibility determination, which was based on the questionnaire administered to new UI claimants, was to screen out claimants who had worked for less than three years on their pre-UI job, some claimants reported shorter work histories on the follow-up interview.

[11] The BLS data pertain to workers age 20 and over who were displaced from their jobs between 1979 and 1983. Individuals were counted as displaced workers if, after holding a job for three years or more, they lost or left their job because of a plant shutdown or relocation, slack work, or the termination of their shift or job.

TABLE III.5

CHARACTERISTICS OF THE ELIGIBLE AND NONELIGIBLE POPULATION

	Eligibles	Noneligibles
Demographic Variables		
Sex		
Male	52.1	59.4
Female	47.9	40.6
Ethnic Group		
White	60.9	56.6
Black	17.2	21.9
Hispanic	19.5	19.7
Other	2.4	1.8
Age		
Younger than 25	0.0	21.6
25-34	30.0	31.8
35-44	26.3	21.4
45-54	21.7	14.5
55-64	18.8	9.0
65 or older	3.2	1.7
Mean	43.2	35.7
Base Period Employment		
Mean Earnings	$18,046	$13,144
Mean Number of Weeks Worked	45.3	40.2
Industry of Main Base Period Employer[a]		
Manufacturing:	47.2	30.2
Durable goods	23.4	13.4
Nondurable goods	23.8	16.8
Nonmanufacturing:	52.8	69.8
Contract construction	5.0	14.7
Transportation and public utilities	6.2	5.9
Wholesale and retail trade	20.2	21.2
Finance, insurance, and real estate	3.0	3.7
Services	15.8	19.9
Other	2.6	4.4

TABLE III.5 (continued)

	Eligibles	Noneligibles
Pre-UI Job[b]		
Mean Weekly Wage	$403	$363
Mean Hours Worked Per Week	41.5	40.5
Months on Pre-UI Job		
Less than 12	2.4	33.1
12 to 35	11.2	35.2
36 to 59	19.7	9.0
60 to 119	32.3	10.9
120 or more	34.5	11.8
Reason Job Ended		
Laid-off	80.5	73.5
Quit	6.1	9.4
Fired	9.5	13.7
Other	3.9	3.4
Reason for Layoff[c]		
Plant or facility closed	26.7	9.8
Company moved	9.3	3.4
Shift eliminated	16.2	10.4
Lack of work	45.1	74.1
Other	2.7	2.3
UI Entitlement		
Weekly Benefit Rate		
$0-$100	7.4	18.3
$101-$150	20.8	27.7
$151-$175	10.1	10.7
$176-$200	9.9	8.6
Over $200	51.8	34.7
Mean Weekly Benefit Rate	$181	$159
Mean Entitlement	$4,531	$3,808
Mean Potential Duration	24.9	23.9
Sample Size	11,060	2,536

NOTE: Figures are the percentage distribution except where noted. Data for the demonstration eligibles pertain to the combined treatment and control groups in the ten demonstration offices. The data for noneligibles are from a sample of noneligibles drawn from the same offices over the same time period as was the eligible sample.

[a] The industry code of the employer listed in the UI data base is reported. When there was more than one base period employer, the industr code of the employer from whom the claimant received the largest amount base period earnings was used.

[b] The data on the pre-UI job come from the interview. The sample sizes are 5,360 eligibles and 469 noneligibles.

[c] The sample for this variable is individuals who were laid-off.

(age and industry) the NJUIRDP population was quite similar to the dislocated worker population identified by the BLS. Both groups comprised substantial fractions of the population older than age 55 (25 percent in the BLS study and 22 percent in the NJUIRDP), and both had large percentages in manufacturing (49 and 47 percent, respectively).

In summary, much of the demonstration-eligible population exhibited the attributes usually associated with the dislocated population and with reemployment difficulties. A substantial proportion of the eligible population were older, a substantial proportion were in manufacturing, and a substantial proportion (about 40 percent) indicated that their plant had closed or moved or their shift had been eliminated. The eligible population also included a large percentage of black and Hispanic workers, groups that often experience labor-market difficulties. Nevertheless, these groups did not account for the entire eligible population. Individuals in the prime of their working lives and individuals from industries which are strong and growing in New Jersey (e.g., the service industry) were also eligible.

The differences in the characteristics of the eligible and noneligible populations described above suggest that the eligibility screens used in the demonstration directed services towards a population who were expected to experience longer unemployment durations and longer periods of UI collection than was the ineligible population. However, it is important that data on labor-market and UI outcomes be examined to determine whether, in fact, the eligible population fared worse than the ineligible population. Data to examine this issue are reported in Table III.6.[12]

These data clearly show that the eligible population did have longer UI durations than did the ineligible population (17.9 weeks versus 15.1 weeks), and this difference is statistically significant. The other measures of UI receipt (dollars collected and the exhaustion rate) also show significant differences between the two groups. Similarly, the employment and unemployment data show the same story. The eligible population took longer to become reemployed than did the ineligible population and, consequently, the eligible population was employed, on average, a smaller proportion of the time in the first year after they began claiming UI than was the ineligible population. Recall rates were also higher for noneligibles than for eligibles.

Thus, the pattern of differences between the eligible group and the ineligible group is fully consistent with the screening objectives. Furthermore, the magnitude of the individual differences is quite large, at least relative to any previously observed program-induced effects on such

---

[12] The data for the eligible sample in Table III.6 pertain to the control sample only.

TABLE III.6

COMPARISON OF THE UI AND LABOR MARKET EXPERIENCE OF ELIGIBLES AND NONELIGIBLES

	Eligibles[a]	Noneligibles
UI Receipt		
Mean dollars paid in benefit year	$3,228	$2,328
Mean weeks paid in benefit year	17.9	15.1
Mean weeks paid in first spell	15.5	11.6
Exhaustion rate	44.7	35.4
Employment and Unemployment		
Mean weeks duration from date of claim to first job or to interview date	31.0	24.9
Mean wages in first year after date of claim	$8,292	$10,206
Mean percent of time worked in first year after date of claim	42.8	52.6
Percent recalled to pre-UI job	20.4	25.6
Sample Sizes[b]		
UI Receipt	2,385	2,536
Employment and Unemployment	1,469	468

---

[a] The control sample is used for this comparison.

[b] The records data sample is used for the UI variables, and the interview sample is used for the post-layoff employment and unemployment variables.

measures. However, while all these comparisons indicate that the eligibility screens did target services toward a group who experienced reemployment difficulties relative to individuals who were not eligible for the demonstration, both groups contained individuals whose experiences were similar to those in the other group. For example, 35 percent of the ineligible population exhausted UI, while 20 percent of the eligible population were recalled to their former employer. Thus, the possibility exists that better targeting could be accomplished in future programs.

IV. INITIAL SET OF COMMON SERVICES

All claimants who were selected as demonstration treatment group members were offered a common set of reemployment services early in their UI claim period. As indicated in the previous chapter, after claimants were selected, they received a letter which notified them to report to the demonstration office for an orientation session. This session was expected to be held in approximately the fifth week after the initial claim. Reporting for the orientation session was required by UI, and nonreporting could have led to the denial of benefits. The orientation session was followed by testing during the same week. A job-search workshop was held the next week, and an individual assessment/counseling interview was held the following week. Reporting for these additional activities was also required by UI unless the claimant was explicitly excused. Claimants who had an assessment/counseling interview were informed at that time about their eligibility for additional services. These additional services differed by treatment group, and it was not until the counseling interview that the claimants knew into which group they had been selected. Until that time, the treatment of claimants in the three treatment groups was uniform.

In this chapter, we examine this initial set of common treatment components; the additional services are examined in Chapters V through VII, and compliance with the reporting requirements is examined in Chapter VIII. In Section A, we begin by describing each of the mandatory service components. Then, in Section B, we report data by local office on the receipt of services and the timing of participation in each of the components. In the final section, we discuss our observations about these service components based on information collected during visits to the local offices.

A. THE INITIAL MANDATORY SERVICES

This section describes the four initial services that were offered to claimants in the demonstration: orientation, testing, the job-search workshop, and the assessment/counseling interview. This core set of services was provided during a three-week period beginning at approximately the fifth week of the UI claim spell.

1. Orientation

As indicated in Chapter III, individuals who were selected for the sample were sent a letter by UI to notify them to report on a specific date and at a specific time to the demonstration office (the local ES office in most cases) for an orientation session. The reporting date was specified for the week after the week in which claimants were selected, so as to give them sufficient time to receive the notice. Schematically, the sequence of events up to the orientation was as follows:

Initial Claim		First Payment	Sample Selection and Notification	Report to Demonstration
Week 1	Week 2	Week 3	Week 4	Week 5

Orientation was conducted as a group session, with approximately 13 to 14 individuals reporting from the 17 to 18 treatment group members called in each week by each office (including those individuals who were attending after having missed a previous session).[1] During the session, the claimants were informed about the initial sequence of demonstration services (testing, the job-search workshop, and the assessment/counseling interview) and were told that additional employment services might be offered to them. They were informed about what they could expect from the demonstration and what was expected of them. They were also registered with the Employment Service if they did not already have an active registration (some individuals had already been to the ES).[2] They were also told when to report for testing and the workshop.

Reporting to the orientation session was mandatory; failure to report was reported to UI. UI was expected to follow up with a fact-finding interview with the claimant and, if an adjudicable issue was identified, a nonmonetary determination.[3] A referral to the next orientation session was also made. The Participant Tracking System was used to report to UI any claimants who did not attend orientation (and other mandatory services). Reports which listed the claimants who were expected to report for orientation (and for the other mandatory services) were generated weekly in each local office through the tracking system. Data on whether or not the claimants did report were then entered into the tracking system, and a Delinquency Report was generated which listed the claimants who did not comply. This report was given to UI, and it formed the basis for its monitoring activities (see Chapter VIII for further discussion).

---

[1] The number called in per week changed over time, and it also varied because some offices did not always have the maximum number of eligibles, including control group members, each week. Over the life of the demonstration, an average of about 17 to 18 persons were called in each week per local site.

[2] UI claimants are generally given an ES reporting date at the time they receive their first payment. Since this time period occurred prior to the selection of claimants for the demonstration and since many claimants who were not selected for the demonstration were required to report to the ES, it was inevitable that some claimants would have been to the ES prior to receiving the demonstration notification letter. In practice, 43 percent of the claimants had been registered with the ES prior to orientation, but this situation did not pose an operational problem.

[3] When a question is raised concerning a claimant's eligibility for benefits an interview is held with the claimant. If that interview indicates that there is reason to examine eligibility (i.e., there is a adjudicable issue) a formal determination of eligibility is made. This determination is termed a nonmonetary determination if it concerns any eligibility rule that does not relate to monetary eligibility requirements.

During the demonstration orientation session, some individuals were excused from all further demonstration services. Most of these individuals were claimants who expected to be recalled but did not have a definite recall date when they completed the New Claimant Questionnaire. Hence, they had been eligible for the demonstration. In some cases, these individuals had more definite recall expectations at orientation. They were excused if they could obtain a letter from their employer to indicate when they would be recalled, although in some offices excusals were made without a letter. During the third month of operations, UI procedures for administering the New Claimant Questionnaire were changed to provide better instructions to individuals who expected to be recalled, so that these cases could more likely be ruled ineligible. The change entailed broadening the definition of "definite recall date" to allow a month's period to count as a definite date (i.e., the actual day for the recall did not need to be known, only the month).

2. Testing

After orientation but during that same week, the Generalized Aptitude Test Battery (GATB) was administered in a group session to the claimants who attended orientation. The purpose of this test, which has been used extensively by the ES, is to compare the aptitudes of individuals with the requirements of many areas of work, so as to facilitate developing a vocational plan for the individual.[4] Individuals with active ES files who had been tested in the last two years were excused from the testing, as were many individuals who were unable to take the test because of language problems or a reading level which was below the minimum level necessary to take the GATB.[5] Claimants also completed an interest inventory, which, together with the GATB results, was used to create a Vocational Information Profile (VIP), equating an individual's aptitude with his or her interests. This profile was used by staff to counsel the claimants. Participation in testing (unless explicitly excused) was also considered to be mandatory by UI.

---

[4] The GATB consists of 12 tests which measure 9 work-related aptitudes. The scores for these aptitudes are aggregated into three components (cognitive, perceptual, and psychomotor), which can then be equated with the needs of specific occupations.

[5] A short pretest was used during the orientation session to assess language and literacy.

3. The Job-Search Workshop

Beginning on the next Monday (i.e., the sixth week), individuals in the demonstration were expected to attend a one-week job-search workshop, which lasted approximately 3 hours each morning. A standard curriculum was followed to ensure that approximately the same workshop was provided in each locality. The goal of the workshop was to ensure that each claimant could define his or her job-search objectives and develop a plan for work search. The standard curriculum included sessions on such topics as dealing with the loss of one's job, self-assessment, developing realistic job goals, organizing an effective job search strategy, and developing resumes and effective job application and interview techniques. The curriculum included both individual activities and group discussions.

Individuals who had attended a standard ES job-search workshop (JSAP) within the previous six months were not required to complete the workshop, nor were individuals who completed a comparable workshop offered by a private vendor (which were generally workshops paid for by the employer at the time of layoff). Other claimants were excused because of language difficulties or literacy deficiencies.

Unless the claimant was excused, his or her participation in the workshop was also considered mandatory by UI, although this requirement did not mean that the individual had to attend every session (although the claimants were not told ahead of time, one absence was permitted). As was the case with the other mandatory services, the names of individuals who did not fulfill the requirement of completing the workshop were to be sent to UI. UI claimants were excused from the UI job-search requirements for the workshop week.

4. Assessment/Counseling

At the end of the workshop, each participant was scheduled for an individual assessment/counseling session, which, except when scheduling difficulties arose, was held during the following week (i.e., approximately the seventh week of the UI claim). For each treatment, this session was to begin with a discussion of the individual's job-search objectives and job-search plan. Counselors were encouraged to review these in conjunction with the test results (the GATB and the VIP scores), and the counselor was to work with the claimant to develop a realistic employability plan. The availability of services, particularly training, also influenced which job objectives were considered to be realistic. The counselor also informed the claimants about the specific additional services that were available to him or her (i.e., training, relocation assistance, and the reemployment bonus). These services differed by treatment group. More specifically, the differences in the content of this interview by treatment group were as follows.

For treatment 1 (JSA alone), the individual was encouraged to use the resource center that had been established in each local office (see Chapter V). The claimants were also informed that, if they did not actively use the center or did not maintain other contact with the staff, they would be contacted and called in on a periodic basis to determine how their job search was proceeding.

For treatment 2 (training and relocation), the possibility of training was introduced, and a discussion was held about its potential advantages to the claimant (see Chapter VI).[6] A decision was then made jointly by the individual and the counselor about whether classroom training, OJT, or job search alone was to be pursued. If classroom training was chosen, the vendor and course were also discussed. Although the counselors were permitted to deny authorization for training or for a particular course, the individual's desires were expected to be given substantial weight in choosing the type of training, and counselors were expected to authorize most training activities. If the claimant chose OJT, the counselor attempted to place the individual in an existing OJT slot or, if none was available, to develop a slot. If an OJT slot was not immediately available, the individual was also to be given a voucher that could be used to inform prospective employers about his or her OJT eligibility. If an employer was interested, a staff member would arrange for an OJT contract if appropriate. However, the voucher was not used extensively. Individuals who chose job search only faced the same requirements as did individuals in treatment 1--they were to use the resource center actively and would be followed up periodically if they had no other contact with staff. Individuals in OJT and classroom training were also expected to use the resource center and comply with call-ins until training was arranged.

The availability of the relocation assistance option was also described to treatment 2 claimants. This option provided financial support for out-of-area job search and for moving expenses, if the move was to accept a job. It was not available to individuals who chose classroom or OJT training.

For treatment 3 (reemployment bonus), the assessment/counseling interview was the same as for treatment 1, except that the reemployment bonus was described. The individual was told about the maximum size of his or her bonus and how its value would decrease over time. The purpose of the session was to ensure that the claimant understood that the sooner he or she obtained a job, the larger the bonus would be. The other eligibility requirements for the bonus (see Chapter VII) were also described.

---

[6] While individuals in treatments 1 and 3 were not offered training services as part of the demonstration, they were permitted to receive training through JTPA if they were Title III-eligible, because they would be eligible for such services in the absence of the demonstration. Thus, individuals who expressed an interest in training were referred to JTPA, but they were not given the extensive introduction to training and were not referred to the demonstration's JTPA staff member who was in the ES office, as was the case for treatment 2 members.

5. Summary of Common Elements

The common set of treatment services occurred in sequence during approximately weeks 5 through 7 after the initial claim date. Schematically, the sequence was as follows:

Orientation and Testing	Job-Search Workshop	Assessment/ Counseling Interview
Week 5	Week 6	Week 7

B. PARTICIPATION IN MANDATORY SERVICES

These four mandatory services were designed to be provided uniformly by the ten demonstration offices. Four mechanisms were used to help achieve this objective: (1) bringing staff from all the offices together for initial training, (2) holding periodic meetings among staff at the same level from all offices (e.g., a meeting among the ten ES counselors), (3) providing a procedures manual to all staff and distributing periodic updates,[7] and (4) monitoring the performance of individual sites through on-site visits and tracking system reports. Despite this attempt to apply the treatments uniformly, the provision of the mandatory services varied somewhat across the sites.

In this section, we present data on the receipt of the mandatory services and the timing of service delivery. These data provide us with an understanding of the overall pattern of services received by all demonstration claimants; moreover, the tables presented in this chapter allow us to analyze differences in participation and timing across local offices.

1. Mandatory Service Receipt

Data on the participation of NJUIRDP-eligible claimants in the initial sequence of mandatory services are reported in Table IV.1. These data show that, overall, 77 percent of the demonstration claimants attended orientation as requested, with 68 percent attending their scheduled orientation session and 9 percent attending a later session. The individuals who attended later orientations were

---

[7] The periodic updates were provided in question-and-answer format and were transmitted to the offices through the tracking system computer network.

TABLE IV.1

RECEIPT OF THE INITIAL REEMPLOYMENT SERVICES, BY OFFICE (percent)

	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
As Percentage of the Total Sample											
Attended Orientation											
Scheduled orientation	63.4	70.5	66.3	70.2	74.6	60.6	69.6	67.5	73.0	65.1	67.9
Later orientation	10.1	6.2	12.5	7.6	6.5	8.8	11.0	11.6	6.1	7.5	8.9
Total	73.5	76.7	78.8	77.8	81.1	69.4	80.6	79.1	79.1	72.6	76.8
Tested	23.2	56.3	10.6	63.2	60.9	33.2	38.5	54.7	62.1	62.7	45.5
Excused from testing[a]	50.0	17.2	63.2	11.9	17.8	32.4	40.6	21.2	13.9	6.2	28.4
Completed JSW	37.8	50.5	48.3	59.4	59.9	49.7	39.6	45.4	58.9	55.1	49.8
Excused from JSW[b]	29.7	19.3	23.8	11.7	11.9	10.4	39.8	24.3	7.7	12.1	19.8
Attended assessment/ counseling interview	58.6	55.5	47.4	50.4	62.7	49.2	61.9	54.5	60.6	52.5	56.2
As Percentage of Those Attending Orientation											
Tested	31.6	73.4	13.5	81.2	75.1	47.8	47.8	69.2	78.5	86.4	59.2
Excused from testing	68.0	22.4	80.2	15.3	22.0	46.7	50.4	26.8	17.6	8.5	37.0
Completed JSW	51.4	65.8	61.3	76.4	73.9	71.6	49.1	57.4	74.5	75.9	64.8
Excused from JSW	40.4	25.2	30.2	15.0	14.7	15.0	49.4	30.7	9.7	16.7	25.8
Attended assessment/ counseling interview	79.7	72.4	60.2	77.6	77.3	70.9	76.8	68.9	76.6	72.3	73.2
Sample Size	816	919	853	581	877	1,049	1,081	939	789	771	8,675

[a] Includes 0.2 percent who were excused because they had previously been tested by the ES.

[b] Includes 0.5 percent who were excused because they had already completed a job search workshop.

generally sent to these sessions by the UI claims examiners when they continued to file claims for UI benefits (see further discussion in Chapter VIII). None of the individual offices deviated very much from this overall pattern. The percentage of the demonstration sample of claimants who attended orientation ranged from 69 percent at Newark to 81 percent at Bloomfield and Elizabeth, representing a range of only 12 percentage points across the ten sites.

Finally, as we discuss in Chapter VIII, a large fraction of the claimants who did not report for orientation stopped claiming UI benefits prior to the scheduled orientation.[8] Most of the others who had their eligibility for benefits questioned had moved to another locale, or had a gap of greater than 5 weeks in their benefit claims period (these individuals were not sent to orientation when they came back into the program).

As mentioned above, some claimants were excused from all services at the time of orientation. Since such excusals were not anticipated in the design, we did not collect explicit data concerning excusals during the demonstration. Instead we examined this issue after the fact by identifying claimants who may have been excused and examining their case files to see if there was information indicating that they had been excused. We defined the population of such potential excusals (from all services) as claimants who attended orientation but who were neither tested nor assessed. Seventeen percent of the claimants who were assessed fell into this group. This percentage varied substantially by office from a low of 6 percent in Bloomfield to a high of 35 percent in Jersey City. In our examination of the files for these individuals we found notations that 18 percent had returned to work, 32 percent were excused because they expected recall, 21 percent were excused for other reasons (language difficulties was the main reason), and 35 percent had either no information or a note indicating that the claimant did not report for services.[9] We also used the interview to examine excusals by asking individuals who did not attend a scheduled service why they did not attend. For each service about half of these individuals said they had become reemployed before the service was scheduled. Expectation of recall was also given as a major reason for not attending.

The data on testing show that almost all claimants who attended orientation were either tested (59 percent) or excused from testing (37 percent). The percentage of claimants who attended orientation but who were neither tested nor excused from testing shows little variation among offices.

---

[8] This was the main reason given in the interview for nonreporting.

[9] We also examined the files to determine if recall or return to work status had been verified with the employer. In many cases no documentation of a verification was found.

However, the excusal rate (which, overall, was higher than anticipated) varied considerably among offices. For example, while Deptford excused less than 10 percent of those claimants who attended orientation from testing, Jersey City, Paterson, Elizabeth, and Newark excused a relatively large percentage (ranging from 46 to 80 percent). These differences in excusal rates arise both because different proportions of claimants were excused at orientation from all services and because different proportions of claimants were explicitly excused from testing in general due to language or reading comprehension difficulties (see further discussion below).[10]

Ninety-one percent of the individuals who attended orientation either completed the job-search workshop (65 percent) or were excused from the workshop (26 percent). While the percentage of individuals who either completed or were excused from the workshop exhibited little variation across offices, the excusal rates for the workshop varied considerably (though less than those for testing). While the rate at which claimants were excused from the workshop was quite low at many of the offices (below 20 percent), the Elizabeth and Paterson offices excused over 40 percent of the individuals who had attended orientation. As with testing, many workshop excusals were granted because of language problems. However, this factor does not appear to explain all of the differences.[11]

In terms of the assessment/counseling interview, Table IV.1 indicates that 56 percent of the claimants who were initially selected for the demonstration continued in the demonstration through this interview. This percentage represents 73 percent of those who attended orientation. The variation across offices in these statistics is not large, although of all the offices Jersey City seems to have experienced the lowest rates of attendance for the assessment/counseling interview in terms of both the total sample (47 percent) and the sample of those claimants who attended orientation (60 percent). This low rate for Jersey City is probably due to the fact that the site appeared to adopt a fairly liberal excusal policy.

Finally, for treatment 2 claimants, the counselors were asked to indicate whether the claimant was interested in classroom training, OJT, relocation assistance, or job search only. This information was recorded at the end of the assessment interview. While this information is quite subjective, it does provide some indication of the extent to which the additional services that were offered to this group

---

[10] Individuals were excused if they had previously been tested; however, these individuals accounted only for a small number of excusals. In addition, one office, Jersey City, did not test claimants whom they felt did not need testing (for example, because they had a well-defined occupation), even though the demonstration rules required that these individuals be tested.

[11] Relatively few were granted because claimants had already attended a workshop.

relative to treatment group 1 were of interest to claimants at the relatively early point in their unemployment spell at which the assessment interview was conducted. Overall, about 60 percent of the treatment group 2 claimants were recorded as having been interested in local job search only, while 29 percent were interested in classroom training, 11 percent in OJT, and 1 percent in relocation assistance. Thus, the percentage interested in additional services was about 40 percent of the total, three-quarters of whom expressed interest in classroom training.

In summary, it is clear that most offices implemented these four services as designed. The major differences among offices appear to lie in the manner in which excusals from all services and from testing and the workshop were granted.

As mentioned earlier, we examined the factors that influenced participation and excusal rates by estimating a set of regressions in which the dependent variables were binary variables that took the value of one if the service was received (or, for some dependent variables, if excusal was granted) and zero if it was not. The explanatory variables represented a common set of variables that we believed might affect participation. These variables included the following characteristics: age, gender, ethnic background, treatment group, base wages, union membership, expected recall, potential duration, weekly benefit amount, pre-unemployment industry, month in which activity occurred (July 1986 through June 1987), and local office.

These regressions yielded a number of interesting conclusions:

- Older claimants were more likely than younger claimants to be excused from testing and the workshop.
- Women were more likely than men to attend each of the four mandatory services.
- Individuals in the "other ethnic group" category (primarily Hispanics) were more likely to be excused from testing and the workshop, but were also more likely to attend counseling.
- Individuals who said that they used union hiring halls to obtain employment and those who expected to be recalled were more likely to be excused from mandatory events and were less likely to attend the assessment/ counseling interview.[12]
- The longer the expected duration of UI receipt and the higher the benefit amount, the less likely the individual was to be excused, and the more likely he/she was to attend mandatory events.
- Individuals who had been employed in nondurable goods industries (as opposed to nonmanufacturing) were more likely to be excused from testing and from the workshop.

---

[12] Six percent of the eligible sample said they used union hiring halls; 36 percent expected recall but did not have a definite recall date.

- In general, the percentage of claimants who attended their scheduled orientation, testing, and the workshop during the first quarter of 1987 (January through March) was low relative to these percentages for the rest of the enrollment period. Conversely, the percentage of excusals was relatively high during this quarter. In addition, the percentage of claimants who attended the assessment/counseling interview was relatively high during the fourth quarter of 1986 (particularly in October and November). These patterns probably reflect seasonal employment patterns which are not controlled for through other variables (such as recall expectation) in the regression.

Moreover, even after controlling for all of these factors, statistically significant differences still exist by office. Thus, the variations among offices observed earlier appear to reflect measured differences in the characteristics of claimants across sites, as well as other, unmeasured characteristics such as, perhaps, how staff members presented the demonstration activities to the claimants and the explicit and implicit excusal policies.

2. The Timing of Service Delivery

The NJUIRDP was intended to be an early intervention program, in the expectation that early intervention would have a greater effect on reducing the length of claimants' unemployment spells and amount of UI collected than would a program that intervened later in the unemployment period. By design, the program was expected to begin providing services at about the fifth week of the unemployment claim, and the core set of mandatory services were to be provided within a three-week period. In this section, we present data that show how these timing objectives were achieved in the demonstration.

The data in Table IV.2 report information on the timing of services. The data show that, overall, orientation generally occurred as planned; the average length of time between the UI date of claim and date of orientation (for those attending orientation) was 35 days, or exactly 5 weeks. Half (50 percent) of the claimants attended orientation sessions that were held during the fifth week after the UI claim while another 34 percent attended sessions that were held in the next week. Because some claimants attended later orientations or experienced delays in receiving their first payment,[13] a small percentage (6 percent) did not attend orientation until the eighth week or later. Nevertheless, the goal of starting service delivery around the fifth week was achieved.

Although these results indicate that this goal of starting service delivery at approximately the fifth week was met (on average, across all demonstration claimants), a great deal of variation existed

---

[13] Claimants who received a first payment longer than 5 weeks after the claim date were not selected for the demonstration.

best copy available

TABLE IV.2

THE TIMING OF INITIAL SERVICES, BY OFFICE

(percent)

	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
Date of UI Claim to Orientation											
29-35 days	48.3	64.7	47.2	58.2	70.0	20.6	35.1	61.9	69.6	28.0	50.0
36-42	29.8	25.3	30.4	27.2	22.8	51.2	44.9	25.0	21.6	53.4	33.5
43-49	11.8	6.4	13.7	8.6	3.7	16.6	12.7	9.2	5.6	12.5	10.2
50 or more	10.1	3.6	8.7	6.0	3.5	11.6	7.3	3.9	3.2	6.1	6.3
Mean days	37.1	33.5	35.6	34.1	33.1	39.1	36.8	33.1	34.1	36.2	35.3
Date of Orientation to Date Tested											
0-7 days	56.5	91.7	81.1	92.6	93.4	85.5	89.7	91.1	89.0	92.9	89.1
8-14	34.4	6.0	8.9	5.7	4.2	9.0	1.9	6.6	8.5	5.2	7.3
15 or more	9.1	2.3	10.0	1.7	2.4	5.5	8.4	2.3	2.5	1.9	3.6
Mean days	5.7	2.4	5.6	2.8	2.5	3.2	3.6	3.0	2.6	1.9	2.9
Date of Orientation to JSW Completion											
0-14 days	88.1	87.7	87.7	80.9	91.0	88.6	87.6	85.5	92.2	83.5	87.6
15-21	9.6	9.1	7.1	12.8	6.5	6.0	7.9	7.8	5.8	10.9	8.1
22 or more	2.3	3.2	5.2	6.3	2.5	5.4	4.5	6.7	2.0	5.6	4.3
Mean days	11.0	11.1	11.8	12.4	10.8	11.6	11.5	12.7	9.8	12.5	11.5
Date of Orientation to Assessment Completion											
0-14 days	50.1	27.1	10.0	9.4	34.3	19.5	19.1	27.4	72.2	17.4	29.2
15-21	40.2	43.2	72.6	69.8	48.5	47.8	42.8	50.0	20.5	60.8	48.3
22-28	5.4	18.1	11.2	14.0	7.9	17.5	22.1	14.1	5.4	15.1	13.4
29 or more	4.3	11.6	6.3	6.8	9.3	15.2	16.0	8.5	1.9	6.7	9.1
Mean days	15.2	19.3	18.5	18.1	18.6	20.3	20.9	18.4	14.9	18.6	18.4

among the offices. While claimants in the Bloomfield and Burlington offices were particularly likely to attend orientation during the fifth week, claimants in the Newark and Deptford sites were most likely to attend orientation during the sixth week. Over 20 percent of the participants at the Paterson, Jersey City, and Newark offices attended orientation after the sixth week.

These differences among sites may reflect a number of factors. First, to some degree, they may reflect differences in the percentage of claimants who attended the scheduled orientation, as opposed to a later session. Paterson and Jersey City, for example, showed a higher percentage of claimants who attended a later session than was the case with most of the other offices. Second, staff at some offices were particularly conscientious in rescheduling claimants when they missed sessions. For example, staff at Burlington sometimes provided special individualized sessions for claimants who missed their scheduled session. Finally, as we found in Chapter III, the data entry of New Claimant Questionnaires lagged in some large offices, particularly Newark, which, in turn, delayed the selection of the sample for some claimants.

The data in the remainder of Table IV.2 show the timing of the remaining mandatory services (for claimants who received those services) relative to the orientation date. As designed, testing occurred during the same week as did orientation for the vast majority of claimants (89 percent), and most claimants who were not tested during the first week were tested during the second week. The average number of days between orientation and testing was slightly greater in the Paterson office (5.7 days) and the Jersey City office (5.6 days) than at the other offices. Claimants in these two offices were more likely than claimants in the other offices to be tested during the second week or later.

The completion date for the job-search workshop was also generally as planned, having been within 14 days after orientation for 88 percent of the claimants who completed the workshop. This pattern is also evident for each of the offices, with very little cross-office variation.

Assessment/counseling interviews were also administered to the majority of claimants (78 percent) within three weeks after orientation. Although it was anticipated that the interview would be administered during the second week after orientation, a larger proportion of all claimants completed the interview in the third week (48 percent) than in the second week (29 percent).

However, this overall pattern for assessment/counseling masks a good deal of variation across offices. Over half of the claimants who completed assessments in the Paterson and Burlington offices completed their interviews within two weeks after orientation, with an average number of 15 days. In contrast, claimants in the Newark and Elizabeth offices completed their assessment/counseling interview an average of more than 20 days after orientation. Over 30 percent of the claimants at these two offices completed their interviews more than 3 weeks after orientation.

Several factors contributed to the delays in some offices in completing assessment/counseling interviews. First, because these interviews were scheduled on an individual basis, claimants and staff had more flexibility in arranging a time that was convenient for both parties (than was true of the other mandatory activities). Second, staff in some large offices had trouble keeping up with the weekly flow of claimants from the workshop. In these cases, central office staff provided back-up support to get back on schedule. Third, the schedules of both the ES counselor and the local JTPA staff person had to be coordinated in order to conduct interviews with treatment 2 claimants.[14]

In summary, the data on the timing of the initial services show that they were generally delivered on schedule, and that the goal of early intervention was achieved. However, some variation did exist among offices, particularly in the timing of both orientation and the assessment/counseling interview.

C. ASSESSMENT OF THE MANDATORY SERVICES

Thus far in this chapter, we have provided a description of the mandatory services as they were intended to be implemented, as well as information on the participation of claimants in these activities. For each of these activities, however, we are also interested in examining how they might have been restructured to make them more useful generally and directed more towards claimants who could benefit from the services. In this section, we focus on these questions. We begin by discussing the process of excusing claimants from all demonstration activities, given that this issue underlies each of the individual activities. We then discuss these issues with respect to testing, the job search workshop, and the counseling/assessment interview.

1. Excusal Policies

A primary issue associated with the demonstration is identifying the individuals who benefit most from the demonstration services. Alternatively, we might consider who is not likely to benefit, and who should thus be excused from services. Although the primary evidence on this issue is obtained from the subgroup analysis presented in the impact and benefit-cost report, our information on how the demonstration was implemented does provide some insight into this issue. The process information provides, first, a description of how the excusal policy was actually implemented and, second, the views of staff members about this issue.

---

[14] In fact, the percentage of treatment 2 claimants who received the assessment/counseling interview more than three weeks after orientation (26 percent) was larger than the percentage for the other two treatments (21 percent and 19 percent for treatments 1 and 3, respectively), and these differences were statistically significant.

As indicated earlier, some claimants were excused from all of the mandatory services after the orientation session. The demonstration rule was that the only persons who could be excused from all demonstration activities were those who expected to be recalled at any time in the future and who presented a letter from their employer to indicate that recall was to occur on or around an approximate date.[15]

In actual practice, the degree to which this standard excusal policy was strictly enforced varied among offices in terms of the criteria that were used to grant excusals and the degree to which expected recall was verified. Although some of the offices followed the standard criteria in granting excusals, several of the offices placed further restrictions on excusals. In particular, these offices granted excusals only to those individuals who expected to be recalled within a specific period of time (ranging from 2 weeks to 4 months). In addition, some individuals were excused from services for reasons other than that they expected recall. The major additional reason for excusals was language difficulty.

A second area in which excusal policies varied across offices was the degree to which and the manner in which expected recalls were verified. Although there was a formal policy which stipulated that recall be verified in writing by the employer, several offices were not always consistent in following this policy. For example, a few sites excused some claimants after talking with the employers by telephone. At least one site excused claimants who obtained a notice from UI which indicated that the claimant was going to be recalled. This notice was based only on the claimant's expectations, and the information was not verified with the employer.

In addition to variations in the standard policy, staff members also had opinions about who should be allowed to participate. In addition to those claimants who expected to return to work, some staff members also suggested that services should be not offered to claimants who were older than 60 years of age or who were pregnant, due to the nature of their employment expectations. Another staff person, in considering whether persons of all ages should be included, suggested that the workshop in particular should have a different format for younger, less experienced claimants. Finally, as we have observed earlier, staff members noted that many individuals could not participate fully in the services due to language barriers.

---

[15] This rule was established early in the demonstration after the initial experience showed that some job attached claimants were selected for the demonstration.

Based on this discussion, it would be useful if the demonstration services are to be replicated, to refine the sample criteria to take these issues into account, and/or to restructure the activities in a manner whereby a broader sample could benefit from them. In addition, the criteria and method of verification of any excusals must be clearly defined and adhered to more generally.

2. Testing

Although our process interview data do not provide concrete answers about who should receive testing, they do provide some insight into both the process that sites actually followed in excusing persons from testing and the thoughts and concerns of staff about this issue.

As indicated earlier, every office excused from testing claimants who had language difficulties or literacy deficiencies. Site staff at several offices also granted excusals to a few claimants with personal emergencies, such as family or health problems. Beyond this information about whom the offices actually excused, staff members also sometimes had strong feelings about who should be tested. For example, one staff member indicated that, because most claimants knew what type of work they wanted and were capable of performing, the test was not very useful for these persons. More specifically, another staff member felt that the persons who benefited most from the testing were those who were less-skilled (and thus were less aware of their abilities).

These comments suggest that the policy of testing all claimants who could take the test may not have been an efficient use of resources. More limited testing at the option of the counselor might have been better. However, if this policy were instituted, it might be necessary to wait until more information on a claimant is known (say, during the job-search workshop) to determine who should be tested. This process might delay the assessment/counseling activity, but the policy of testing most individuals immediately after orientation may still be worthwhile, given that the marginal administrative cost of testing additional claimants is low once testing sessions are set up.

3. Job Search Workshop

Of all the initial services, the job search workshop received the greatest enthusiasm from both claimants and staff members.[16] Staff members noted that, although some claimants were not interested in the workshop on the first day, their interest in the workshop generally increased as the week progressed. Despite such enthusiasm, several questions about the workshop can be raised: Were the goals of the workshop met? Was the composition of the group who attended the sessions appropriate? Who should receive the workshop? How long should the workshop last?

---

[16] Interview data indicated that claimants who felt the program was useful considered the workshop the most useful element.

As indicated in the first section of this chapter, the goal of the workshop was to help each claimant define his or her job-search objectives and develop a plan for work search. An issue of immediate interest in the process analysis was whether or not this goal was achieved. At each of the offices, some attention was paid during the workshop to developing job search objectives and plans. However, the degree to which this was done and the formality of these plans varied considerably among offices; some offices required written plans that were reviewed in detail by the workshop leader, and others required only that claimants think about their plans. In addition, a few offices developed plans only if time was available on the last day of the workshop. We believe that more attention to developing a specific job search plan would be worthwhile.

A second issue that can be addressed with the process data pertains to who should participate in the job search workshop. Although individuals with language or literacy difficulties were generally excused, some sites felt strongly that individuals should participate if they could benefit at all from the experience, and these sites were less willing to excuse anyone. Another issue associated with participation is that some local office staff felt that the goal itself was not appropriate for claimants who already had developed job-search objectives and plans prior to attending the workshop. A few claimants so strongly believed that they did not need the workshop that they continually disrupted it. One staff member noted that the persons who benefited most from the workshop were those who had not been expecting to lose their employment and, thus, had not had the time to develop job search plans.

Although the stated goal may not have been met fully for all workshop participants, the process data indicate that the workshop was likely to have generated other benefits to a potentially wider, cross-section of participants. In particular, staff felt that the workshop was important in terms of enabling claimants to share their problems with others in similar circumstances. Indeed, several staff members and claimants stressed the cathartic benefits of the workshop. Claimants provided support and job search advice to each other and sometimes developed relationships that carried beyond the workshop sessions.

A third issue pertains to determining the appropriate composition of the group that participates in a workshop. Some sentiment was expressed by both staff and claimants that the workshops should not contain a mixture of persons who had different work experience. This sentiment reflects, at least in part, the belief that the job search concerns and techniques of different occupations require different curricula. For example, some staff members indicated that the demonstration workshop materials were more appropriate for individuals who were looking for white-collar work (for which a resume is required, for example) than for blue-collar work. Other staff welcomed the disparate types of claimants and sometimes utilized white-collar claimants in the workshop. For example, in one workshop, a claimant who had been in personnel played the role of the interviewer in mock interviewing sessions. Whether one agrees or not with the desirability of having occupation-specific workshops or, at least, separate workshops for blue-collar and white-collar occupations, the scale of operations in each office was such that only one workshop could be held each week. Thus if more than one type of workshop had been offered, service delivery would have been delayed for some claimants and the goal of early intervention would have been compromised.

Finally, staff and claimants provided feedback about the desirable length of the workshop. A number of staff members felt that the workshop was too long, and that 3 or 4 half days would be adequate; others suggested that it be reduced to one full day and three half days to provide staff with a full day to complete other tasks. Conversely, some claimants and staff felt that the workshop should have been longer. Indeed, one staff member noted that the allotted time was not adequate to set job search goals and develop realistic plans. Overall, our judgment is that at least four half days were necessary to achieve the goal of having workshop participants develop a work search plan.

4. Assessment/Counseling Interview

The goals of the assessment/counseling interview were (1) to review the claimant's job-search objectives and plan (as developed in the workshop) in conjunction with the test results in order to develop a realistic job-search plan and (2) to inform the claimant about the specific additional demonstration services that were available to him or her (i.e., training, relocation, the bonus, etc.). In general, these goals were accomplished. However, the degree to which an intensive counseling session occurred depended primarily on the needs of the claimant. As indicated in our discussion of the job search workshop, many claimants had already developed a job search plan with which they were happy and which fit their abilities. Counseling for these claimants was essentially unnecessary.

However, for those claimants whose plans were not as clear, staff felt that the one-to-one counseling session was essential. The session (more so than at any of the other sessions which were group-oriented) allowed staff to focus on individual strengths, weaknesses, interests, concerns, and plans. However, limitations were imposed on the process. Some staff members indicated that they realistically had very little time to spend with each individual in the counseling session. In addition, staff at least two offices indicated that, due to the layout of the offices, there was not very much privacy in which to conduct these counseling sessions, thus limiting the degree to which claimants and staff felt comfortable about discussing personal concerns.

5. Conclusions

In summary, both program staff and claimants (as reported on the follow-up interview) expressed the view that the job-search workshop was the most useful service provided by the demonstration.[17] The assessment/counseling interview was also mentioned as useful, but to a lesser degree. In addition, to these overall assessments three other general observations about the initial services can be made.

First, staff and to some extent claimants expressed the view that the services were either not needed or were not useful to some claimants. In particular, staff argued that claimants who had well developed plans for their job search did not need the services while those who did not have well developed plans could benefit from the services. This observation suggests that these services might be made optional to claimants or that future program planners might consider a more tailored approach to providing services that takes into account a claimant's job experience, career interests, work-search plan, and perhaps language proficiency and literacy levels. However, the impact and benefit-cost report concluded that the mandatory nature of the initial services was an important factor contributing to program impacts. That report also indicated that the services had a greater impact on individuals with marketable skills than on individuals facing more hard core unemployment problems. Since individuals with marketable skills may be those with job search plans, making services optional might reduce the impact of the program.

Second, when claimants were asked how the program could have been more helpful, they frequently indicated that the program would have served them better if it provided more referrals to well paying jobs. Some claimants felt that the focus on self-directed job search was inappropriate, and that they would have preferred being sent directly to prospective employers.

Finally, the fact that some claimants were excused from testing, the workshop, or, indeed, all services due to language or reading comprehension difficulties suggests that a greater availability of English as a Second Language (ESL) or remedial education services might have been useful for these individuals.[18] The explicit provision of such services was not part of the NJUIRDP design, and although referrals to such services could be made, it appears that few individuals did receive such services.[19] Future programs might want to consider providing such services particularly if they are implemented in areas such as New Jersey that have high concentrations of Hispanics or other groups that may not speak English. For example, although we did not record the reasons for excusals from testing or the workshop in the New Jersey demonstration, tabulations which we have done suggest that as many as 8 to 10 percent of the treatment groups members may have been excused from testing or the workshop for language or reading comprehension difficulties.[20]

---

[17] About 60 percent of the claimants said that the services provided in the NJUIRDP were helpful in finding a job.

---

[18] Several respondents to the follow-up interview expressed interest in receiving ESL training when asked what additional services they would have wanted to have received.

[19] For example, the JTPA data system indicates that less than 0.2 percent of the members of any treatment group received English as a Second Language (ESL) or remedial education (the ES data system does not explicitly record provision of these services). Moreover, there was only one Hispanic who was excused from testing who received such services.

[20] This estimate was made by excluding Jersey City from the computation (since that office adopted an expanded excusal policy) and by assuming that individuals who were excused from testing or the workshop and who expected recall or who used union hiring halls were excused for those reasons. The remaining individuals who were excused were considered to have potential language or reading comprehension difficulties. About 40 percent of these individuals were Hispanic.

V. PERIODIC JOB SEARCH ASSISTANCE

Claimants who received the assessment/counseling interview at the end of the initial period of service receipt were offered additional services to help them become reemployed. These additional services were offered at approximately the seventh or eighth week of their UI claim spell. The set of services that were offered differed among the three treatment groups, but all claimants (with the exception of those who entered training) were informed that they were expected to maintain periodic contact with the demonstration office, either directly with staff to discuss their job search activities or by using a resource center that was situated in each office. The resource centers contained job-search materials and equipment, such as job listings, telephones, and occupational and training literature. Claimants were encouraged to use the resource centers actively, and they were informed that if they did not come to the office periodically they would be contacted by ES staff and asked to do so. These periodic follow-up contacts were scheduled to occur at 2, 4, 8, 12, and 16 weeks following the assessment interview.

In this chapter we examine the periodic JSA follow-up activities in the ten local offices to determine how well the procedures outlined above were implemented. We begin with a more complete description of the nature and purpose of the JSA follow-ups and of the methods used by staff to monitor follow-up activities. We then examine data that show the extent of follow-up activities achieved in the demonstration, and consider the nature of the follow-ups, particularly the use of the resource center. The final section provides a brief summary and assessment of the follow-up activities.

A. THE RESOURCE CENTER AND JSA FOLLOW-UP ACTIVITIES

An important objective of all three treatment packages was to encourage all claimants to engage in on-going, intensive job search, with the exception of those in treatment 2 who entered training. The design of the NJUIRDP attempted to promote this continued active job search in two ways: (1) by establishing the resource centers and (2) by requiring follow-up contacts with claimants.

The resource center set up in each office was to provide a supportive environment that would help claimants in their job search efforts. Ideally, each center was expected to provide four ingredients useful to claimants' job search: (1) a place for claimants to come to look for work, (2) materials useful for job search efforts, (3) staff support if necessary, and (4) support from the claimants' peers.

Claimants were encouraged to use the centers when they were administered the assessment/ counseling interview. Attendance was monitored with a sign-up sheet.

In terms of the first ingredient, a physical space was designated as "the resource center" in each office. Initially, it was hoped that a small room or partitioned space could be found and used as the resource center, but such space was available only in four sites. In the others, the "resource center" was a desk(s) or table located in the office, generally next to the desks used by demonstration staff.

A list of materials useful for job search was prepared, and each office was expected to make these materials available in the resource center. The materials included, among other items, occupational information, industrial directories, telephone books, job-opening announcements, area and local newspapers, training information, and miscellaneous other useful materials, such as bus and train schedules. One or two telephones and a microfiche reader to examine Job Service orders were also expected to be made available.

Because of resource constraints, it was not possible to staff the resource centers, but it was expected that staff could be located close enough to the centers that they would be available to show claimants how to use the microfiche readers or other materials. In several offices, specific times were also established to make staff (generally the part-time interviewers) available in the resource center.

Finally, it was hoped that the level at which the resource centers were used would be such that claimants could interact with each other and provide complementary support to their job search efforts. Although peer-group support did occur occasionally in some centers, the level of activity was not sufficient to generate much interaction among claimants (as we discuss below).

In addition to the resource center, the JSA follow-up component also required that claimants who continued to collect UI maintain periodic in-person contact with demonstration staff. The periodic contacts were monitored by demonstration staff, and staff were expected to provide assistance and encouragement to the claimants during their on-going job search efforts. To do so, each demonstration office used the Participant Tracking System to generate a weekly list of all individuals who completed the assessment interview 2, 4, 8, 12, and 16 weeks previously and who were still claiming UI.[1],[2] Demonstration staff were to review these lists to identify individuals who used the resource center actively during the previous two weeks, as well as those with whom they had direct in-person contact regarding their work search activities. All other claimants were to be contacted and asked to come in to the office to review both their job search activities and the offerings of the resource center. The contacts were to be made by telephone initially, but individuals who did not report after a telephone contact were to be sent a specific reporting time by mail. Individuals who failed to comply with a specific call-in time were to be reported to UI using the standard ES/UI reporting mechanism that was in place prior to the demonstration. This reporting mechanism entailed sending a form (the ES 572) to UI, which provided information to UI staff that might lead to an examination of UI eligibility (see Exhibit V.1).

---

[1] This last follow-up occurred at approximately the 24th week of UI collection.

[2] Claimants who entered training or who had an expected date of entering training within three weeks were not included on this list. They were not expected to continue follow-up activities.

As we discuss later, some local offices modified these demonstration reporting requirements--primarily by making, at the assessment/counseling interview, an initial 2-week follow-up appointment with staff (thereby deemphasizing the claimants' independent use of the resource center), but also by conducting follow-ups by telephone (thereby removing the requirement that claimants come in to the office for their periodic follow-up contact).

In general, the purpose of the periodic JSA follow-ups was to maintain contact with claimants to provide job search guidance and to let them know that the staff were interested in their job search activities and prospects. The procedures also provided a set of tangible job search requirements that supplemented the regular bi-weekly reporting of job search contacts to UI. However, compared with the initial set of mandatory demonstration services that were discussed in the previous chapter, monitoring during the follow-up period was generally loose, and, because of the design, the reporting of noncompliance to the UI system was not as regimented. For the initial services, a Delinquency Report was generated from the Participant Tracking System which listed all claimants who had not complied with the reporting requirements, and this report was sent to UI. A similar report did not exist for the follow-ups; instead, ES staff were expected to report noncompliance to UI on an individual basis when a potential UI eligibility issue arose.

B. EXTENT OF JSA FOLLOW-UP

In this section, we examine the degree to which claimants who received the assessment/counseling interview maintained contact with the demonstration through the five potential follow-ups that occurred at 2, 4, 8, 12, and 16 weeks after assessment. The data in Table V.1 provide estimates of the extent to which claimants who attended assessment were expected to maintain contact with the demonstration. Those who stopped collecting UI or who entered classroom training or were scheduled to enter

EXHIBIT V.1

ES 572

APPLICANT DATA		
NAME		SOCIAL SECURITY NO
DOT CODE	JOB TITLE	UI CLAIMS OFFICE
LAST SALARY (FOR B, C & D ONLY)		DATE

☐ **A. CALL IN**

Call in notice sent ____________
(Date)

☐ Failed to respond
☐ Returned attached card

☐ **B. NOT REFERRED**

☐ Will be returning to work ____________
(Date)

at ____________
(Name of Company)

☐ Hours of work
☐ Attending school
☐ Will attend school
☐ Leaving the area
☐ Non-Citizen without work permit

☐ Restricting Wage/Salary to $ ____________

☐ Restricting to ____________
(Type of Work)

☐ Distance ____________
(Time Limitation)

☐ Other ____________

NJES-572 (R-4-79)

☐ **C. REFUSED REFERRAL**

Refused referral on ____________
(Date)

☐ Distance ____________
☐ Transportation ____________
☐ Salary ____________
☐ Type of work ____________
☐ Hours ____________
☐ Other ____________

☐ **D. JOB REFERRAL**

Referred to job and — Date

☐ Failed to report to interview ____________
☐ Refused employer's offer ____________
☐ Failed to report for work ____________
☐ Hired ____________

JOB DATA (C & D)					
NAME OF EMPLOYER					
ADDRESS					TELEPHONE NUM
TYPE OF JOB					SALARY
START	______	AM	______	PM	HOURS PER DAY
END	______	AM	______	PM	HOURS PER WEE
ADDITIONAL INFORMATION					
DATE	SIGNATURE				

TABLE V.1

ASSESSED CLAIMANTS WHO WERE EXPECTED TO REPORT FOR JOB-SEARCH ASSISTANCE FOLLOW-UP ACTIVITIES

	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
PERCENT											
2-Week Follow-Up	88.7	89.8	95.3	91.2	91.5	91.9	90.0	92.8	86.2	93.3	90.9
4-Week Follow-Up	82.4	85.3	88.4	83.5	86.4	84.9	84.5	86.5	77.4	87.4	84.6
8-Week Follow-Up	70.5	73.1	77.2	76.1	76.0	70.0	71.5	73.2	64.0	77.3	72.7
12-Week Follow-Up	59.2	59.6	58.9	64.4	61.8	53.9	56.1	59.0	48.5	62.7	58.1
16-Week Follow-Up	43.3	47.5	43.6	51.3	47.3	39.3	39.5	43.8	35.1	46.4	43.3
NUMBER											
2-Week Follow-Up	424	458	385	320	503	474	602	475	412	378	4,431
4-Week Follow-Up	394	435	357	293	475	438	565	443	370	354	4,124
8-Week Follow-Up	337	373	312	267	418	361	478	375	306	313	3,540
12-Week Follow-Up	283	304	238	226	340	278	375	302	232	254	2,832
16-Week Follow-Up	207	242	176	180	260	203	264	224	168	188	2,112
SAMPLE SIZE	478	510	404	351	550	516	669	512	478	405	4,873

NOTE: The number expected to report for job-search assistance follow-up was estimated by assuming that anyone who claimed UI benefits after the call-in date and who was not in training was expected to report. This estimate probably overstates the number who were expected to report, because some individuals may have had a break in their UI claims and may not have been claiming benefits at the reporting date.

within 3 weeks after the follow-up date were not expected to report.[3] Included in the percentage expected to report were any claimants not in these categories who claimed benefits after the JSA reporting date. Since the UI claims of some individuals contain gaps, not all such individuals were actually claiming benefits at the call-in date. Thus, these data probably overestimate the percentage who were actually expected to report.

With this caveat in mind, the estimate shows that 91 percent of the claimants were expected to report to the demonstration at the 2-week follow-up, and 85 percent at the 4-week follow-up. The percentage expected to report declined steadily (about 14 percent for each four-week period) as claimants stopped collecting UI. About 43 percent were still expected to report at the 16-week follow-up (the fifth follow-up). The data also show some small differences by site. These differences are due both to the different strength of the local economies and to differences in the success of the local sites in helping claimants become reemployed.

Data reported in Table V.2 show the degree to which the claimants who were expected to report to the demonstration complied with the requirement. As shown in the table, 92 percent satisfied the first follow-up requirement (i.e., the 2-week follow-up), while a smaller, although still large, percentage satisfied the remaining follow-up requirements. About 80 percent are listed as satisfying the final 16-week follow-up requirement. Most of the individuals who satisfied the follow-up requirements were recorded as not needing a call-in, because they had made an in-person contact with demonstration staff at approximately the reporting date. A considerably smaller percentage were actually called in and satisfied a call-in requirement. The percentages who satisfied the JSA follow-up requirements are similar among most sites, although variation exists among the sites, particularly for the later follow-ups. Burlington is the site that consistently showed the highest percentage who satisfied the reporting requirements (97 percent or higher for each follow-up), while Bloomfield and Newark showed the lowest. However, the percentage who satisfied the reporting requirements in these two sites was not that much lower than the average for the first several follow-ups, and even the single lowest percentage who satisfied a reporting requirement (the 16-week follow-up in Newark) was 58 percent. Substantially greater variation exists among the sites in terms of how the follow-ups were satisfied; a few sites appear to have used formal call-ins (Hackensack and Jersey City), while most sites did not. These

---

[3] In practice, some of these individuals were in fact included on the call-in lists and were called in because they were still claiming UI just prior to the follow-up date. To program operators, they appeared to be active claimants.

TABLE V.2

JOB-SEARCH ASSISTANCE FOLLOW-UP ACTIVITIES FOR THOSE EXPECTED TO REPORT:
PERCENT WHO SATISFIED THE REPORTING REQUIREMENTS

	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
2-Week Follow-Up	98.4	93.5	98.4	92.2	87.5	84.0	91.7	88.6	99.3	88.4	91.9
No call-in needed	88.4	16.9	36.6	84.1	82.5	74.7	73.6	74.7	93.9	84.4	70.7
Call-in completed	10.0	76.6	61.8	8.1	5.0	9.3	18.1	13.9	5.4	4.0	21.2
4-Week Follow-Up	96.7	93.1	96.1	89.8	84.0	84.7	87.1	84.7	99.2	86.4	89.8
No call-in needed	82.0	12.4	36.4	85.0	78.5	69.4	71.2	63.9	94.3	81.4	66.8
Call-in completed	14.7	80.7	59.7	4.8	5.5	15.3	15.9	20.8	4.9	5.0	23.0
8-Week Follow-Up	95.0	92.2	93.6	91.0	79.4	78.4	87.0	82.7	98.7	82.1	87.5
No call-in needed	79.2	11.5	32.4	88.0	74.6	59.0	71.1	59.7	95.4	80.2	64.4
Call-in completed	15.8	80.7	61.2	3.0	4.8	19.4	15.9	23.0	3.3	1.9	23.1
12-Week Follow-Up	95.1	92.8	85.3	88.9	72.1	71.6	83.7	79.5	97.8	77.2	83.9
No call-in needed	85.2	13.8	32.4	87.2	70.0	47.1	69.1	59.9	94.4	76.0	62.8
Call-in completed	9.9	79.0	52.9	1.7	2.1	24.5	14.6	19.6	3.4	1.2	21.1
16-Week Follow-Up	92.3	94.2	79.0	87.8	66.5	57.6	84.1	73.2	97.0	73.4	80.2
No call-in needed	76.8	16.5	22.7	87.2	65.0	41.4	46.6	54.9	94.6	72.9	60.3
Call-in completed	15.5	77.7	56.3	6.0	1.5	16.2	37.5	18.3	2.4	5.0	19.9

NOTE: The sample for each call-in are individuals whose UI claim date was after the call-in date who were not in training. Sample sizes are shown in the lower panel of Table IV.1.

"call ins," particularly the 2-week call-in, were sometimes appointments made in advance, rather than call-ins that were necessary when a reporting period was missed. Other sites used a less formal procedure whereby claimants were informed to report to ES when they went to UI to receive their check. This procedure seemed to be adopted when the UI and ES offices were co-located, whereby claimants could easily visit the ES when they went to UI.[4]

In terms of nonreporting (see Table V.3), we find that, overall, 3 to 4 percent of the total number of claimants who were expected to report at each follow-up were called in but did not report. This percentage was quite small in most offices, with the exception of Newark and Perth Amboy, in which a relatively large percentage of claimants appear not to have reported when called in. When claimants did not report as directed, the ES-572 form (described earlier) was to be sent to UI. This form, or report, was to generate a review of the claimants' UI eligibility if the circumstances warranted doing so. Some ES-572s were sent at each call-in, but the numbers were generally low (371 for the entire demonstration), and two offices (Perth Amboy and Deptford) did not send any reports.[5] ES-572s appear to have been sent to UI in about 33 percent of the cases in which claimants did not report. Others were sent for claimants who eventually reported.

The remaining cases appear to have had no follow-up activities associated with them. The no-follow-up rate ranged from 5 percent for the 2-week follow-up to 16 percent for the 16-week follow-up. These rates of no follow-up varied by site, but were consistently low in Paterson, Hackensack, Perth Amboy, and Burlington. Bloomfield and Deptford were consistently higher than the other sites, while the remaining sites exhibit low rates for the initial follow-ups and higher rates for the later follow-ups.

These data on the extent of follow-up indicate that the goal of maintaining on-going contact with claimants was fulfilled very well in most sites and adequately in all sites. However, this rosy picture must be tempered somewhat by noting that not all follow-ups occurred in a timely manner. On average, call-ins were scheduled 10 to 16 days after the 2-week, 4-week, or other reporting date, which seems reasonable given that, except when formal appointments were made in advance, claimants were generally not called in until they missed a reporting date. These call-ins were completed an average of about one day after they were scheduled. This average, however, varied considerably by office. The

---

[4] New Jersey handled UI claims through a bi-weekly in-person reporting schedule during the demonstration period.

[5] The Hackensack, Jersey City, Newark, and Elizabeth sites were the most active in sending ES-572s.

TABLE V.3

JOB-SEARCH ASSISTANCE ACTIVITIES FOR THOSE EXPECTED TO REPORT: PERCENT WHO DID NOT SATISFY REPORTING REQUIREMENTS

	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
2-Week Follow-Up	1.6	6.5	1.6	7.8	12.5	16.0	8.3	11.4	0.7	11.6	8.1
Did not comply with call-in	0.9	2.0	1.6	1.6	0.2	11.6	2.8	7.2	0.7	0.0	3.0
No follow-up	0.7	4.5	0.0	6.2	12.3	4.4	5.5	4.2	0.0	11.6	5.1
4-Week Follow-Up	3.3	6.9	3.9	10.2	16.0	15.3	12.9	15.3	0.8	13.6	10.2
Did not comply with call-in	1.8	2.3	1.7	1.7	0.0	7.3	4.9	12.0	0.8	0.9	3.6
No follow-up	1.5	4.6	2.2	8.5	16.0	8.0	8.0	3.3	0.0	12.7	6.6
8-Week Follow-Up	5.0	7.8	6.4	9.0	20.6	21.6	13.0	17.3	1.3	17.9	12.5
Did not comply with call-in	2.7	2.4	0.3	0.0	0.0	7.5	3.6	14.7	0.6	0.3	3.4
No follow-up	2.3	5.4	6.1	9.0	20.6	14.1	9.4	2.6	0.7	17.6	9.1
12-Week Follow-Up	4.9	7.2	14.7	11.1	27.9	28.4	16.3	20.5	2.2	22.8	16.1
Did not comply with call-in	1.0	2.0	2.1	0.0	0.0	6.8	2.4	17.9	0.9	0.4	3.5
No follow-up	3.9	5.2	12.6	11.1	27.9	21.6	13.9	2.6	1.3	22.4	12.6
16-Week Follow-Up	7.7	5.8	21.0	12.2	33.5	42.4	15.9	26.8	3.0	26.6	19.8
Did not comply with call-in	2.9	1.2	2.8	0.0	0.0	8.4	2.3	23.2	0.0	0.0	4.2
No follow-up	4.8	4.6	18.2	12.2	33.5	34.0	13.6	3.6	3.0	26.6	15.6

NOTE: The sample for each call-in are individuals whose UI claim date was after the call-in date who were not in training. Sample sizes are shown in the lower panel of Table IV.1.

average lag from the reporting date to the call-in was under one week in most offices, while Paterson, Elizabeth, and Hackensack averaged 2, 3, and 6 weeks, respectively. Hackensack was the office that appeared to rely more on call-ins than any other office.

Another factor that should be noted is that several sites conducted follow-ups by telephone and did not require claimants to report in-person. This practice was probably more prevalent early in the demonstration. Once the initial problems associated with the selection of and initial service delivery to claimants were addressed, central office staff focused more heavily on ensuring that the follow-ups were completed as designed.

Despite the lags in some call-ins and the fact that not all follow-ups were conducted in-person, we believe that, when compared with the experience of ongoing employment and training programs, the degree of follow-up in the demonstration was quite high. Few such programs even have systematic follow-up procedures, let alone procedures that include multiple in-person follow-up calls. For example, none of the six dislocated worker demonstration projects evaluated in Corson et al. (1984) included a systematic follow-up. Instead, participants were contacted if job referrals were available, and, in some of the demonstrations, participants who had not been active users of job clubs or resource centers were contacted on a one-shot basis to determine whether they had become reemployed.

C. THE NATURE OF THE JSA FOLLOW-UP

In the previous section, we indicated that the majority of claimants who satisfied the follow-up requirements were listed as not requiring a call-in. Call-ins were unnecessary when claimants had already been in contact with demonstration staff, either by having used the resource center or by having made other contacts with staff. Table V.4 shows the reasons that were recorded by staff when no call-in was needed, listed by the 2-week, 8-week, and 16-week follow-ups. These data show that 24 percent were recorded as active resource center users for the 2-week follow-up, and that this percentage remained under 30 percent for the other follow-ups. For the majority of cases, "other contact with staff" was given as the reason for no call-in necessary. Some were also recorded as having stopped collecting UI. Although we included in these tables only those claimants whose UI claim week end date occurred after the follow-up date, it is possible that some claimants had stopped collecting at the time of the follow-up and resumed collecting UI later; however, it is unlikely that this occurrence explains all of these cases.

TABLE V.4

DISTRIBUTION OF REASONS THAT NO FOLLOW-UP CALL-IN WAS NECESSARY

	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
2-Week Follow-Up											
Resource center user	43.5	19.7	0.7	1.8	12.5	13.8	27.5	1.4	49.1	45.8	23.8
Other contact	53.1	59.2	92.2	94.9	85.3	84.2	70.2	97.2	47.8	48.3	72.7
Stopped collecting UI	3.5	21.1	7.1	3.3	2.2	2.0	2.3	1.4	3.1	6.0	3.5
8-Week Follow-Up											
Resource center user	47.9	15.9	5.0	3.8	5.8	24.9	44.7	14.3	50.7	46.2	29.3
Other contact	40.8	40.9	77.2	92.4	89.1	68.5	52.4	79.0	39.4	44.2	62.7
Stopped collecting UI	11.2	43.2	17.8	3.8	5.1	6.6	2.9	6.7	9.9	9.6	8.1
16-Week Follow-Up											
Resource center user	37.1	2.5	7.5	0.0	1.8	32.1	44.7	4.1	52.8	40.8	25.9
Other contact	28.9	32.5	77.5	97.5	93.5	50.0	48.1	81.3	30.8	37.2	58.2
Stopped collecting UI	34.0	65.0	15.0	2.6	4.7	17.9	7.3	14.6	16.4	21.9	15.9

NOTE: The sample for each call-in are individuals whose UI claim date was after the call-in date who were not in training and who were recorded as not needing a call-in. The sample sizes are the number who were expected to report as listed in Table IV.1, multiplied by the percentage with no call-in needed (Table IV.2).

These data suggest that, in many sites, the resource centers were not the primary vehicle through which claimants maintained contact with the demonstration. Although these data may obscure some visits to the resource center when multiple visits or multiple-purpose visits occurred within a reporting period, say, to an interviewer and to the resource center (since only one type of contact could be recorded in the tracking system database), our site visits, and the interview data suggest that the first conclusion is probably warranted. The interview data indicate that 34 percent of the claimants who were assessed and might have used the resource centers did not know about them while 40 percent indicated that they used the centers. A number of these individuals said that the centers were not useful because they did not have suitable job openings.

The site-by-site data in Table V.4 also support this conclusion. They show that only four sites--Paterson, Elizabeth, Burlington, and Deptford--had more than a modest level of resource center use. In contrast Jersey City, Butler, and, to some extent, Perth Amboy had very low levels of resource center use.[6] Based on our site visits, the primary difference between sites in which claimants used the resource centers and those in which they did not was the degree to which staff promoted resource center use during both the initial service period and subsequently in follow-up visits. The extent to which staff promoted resource center use was also evident in the degree to which the job search materials made available by the state were supplemented by local staff. The resource centers in most sites were poorly stocked initially, and several continued to lack some of the required equipment (e.g., telephones and microfiche readers) throughout the demonstration, but centers that were promoted and used tended to have the "extras" (e.g., up-to-date newspapers, bus and train schedules, maps, etc.) that were not supplied centrally and required local initiative. Although the physical layout of a number of the centers was also less than ideal (merely a table in an open space in the office), these problems with physical layout appeared to be relatively unimportant to the successful use of the centers. One of the most widely used resource centers (Elizabeth) contained only a table and several chairs in an open area, but claimants in this office did come in to peruse the available materials and talk with demonstration staff. In this site also, enough claimants used the center that some interaction among claimants occurred. In general, greater interaction among claimants might have occurred if the population served by individual centers had been larger. In summary, the centers appear to have been used when the potential utility of the centers was emphasized during the assessment/counseling interviews; when it was not, the centers were not used to any great degree.

---

[6] It should be noted that severe space constraints in the Butler office made it impossible to make space that could be accessed at any time available for the resource center. Consequently, the resource center materials in this office were only available for claimant use on a very limited basis, one afternoon a week.

D. SUMMARY AND ASSESSMENT

Our review of the periodic JSA follow-up procedures used in the NJUIRDP indicates that the sites generally achieved the goal of maintaining ongoing contact with claimants throughout their UI claims spells. The purpose of this contact was to provide job search guidance and assistance if necessary, and to let claimants know that the staff were interested in their job search activities and prospects. However, our review also indicates that these periodic contacts did not always follow the strict schedule (2, 4, 8, 12, and 16 weeks after assessment/counseling) that had been laid out in the design, nor were all the contacts made in-person as desired. The monitoring activity was also fairly benign, in that the failure of claimants to report was not routinely reported to UI.

The resource centers appear to have been used fairly extensively in some of the offices but not in others. The most important distinction between these two types of offices seemed to be the attitude of staff: if staff promoted using the centers, they were used; if they did not, they were not.

This brief summary raises several questions about JSA follow-up procedures as they might be implemented in future programs. First, were the five follow-ups too many? Could the objective of maintaining contact with claimants be achieved with a smaller number of follow-ups? Second, did the contacts need to be in-person? Would telephone contacts have been sufficient? And, third, how should the resource centers be utilized?

Ideally, answers to these questions require an examination of how variation in follow-up procedures (for example, in the number of follow-ups) affects the behavior of claimants. However, since the NJUIRDP did not test alternative follow-up procedures, this analysis is not possible. Nevertheless, several observations can be made based on our site observations.

The intensity of follow-up as reflected in the number of follow-ups and the requirement that they be made in-person could probably be lessened somewhat while still maintaining contact with claimants. Moreover, giving staff discretion about the number of follow-ups necessary for individual claimants and their nature could also be beneficial, because staff resources could then be directed towards the claimants who could use them the most. However, some specific requirements are probably necessary so that the performance of staff can be monitored against a minimum standard. As we saw in the demonstration, some "shortcuts" were adopted in some cases even when the requirements were quite specific. It would also be useful, as was the case with the tracking system in the demonstration, to adopt a mechanism for notifying staff about which claimants should be followed up. Requiring that some follow-up information be collected and reported would also facilitate monitoring the follow-ups by supervisory staff. This monitoring was important in the demonstration, and would also be important in an ongoing program.

Finally, while the resource centers were not used extensively in all sites, the concept should not be dismissed out of hand by future program designers. As we indicated, some sites did use resource centers fairly intensively as the focal point for claimants' visits. In addition, the centers could probably be improved if they served a larger population. In that case, more space, equipment, and staff could be justified for a single center, and more claimants might visit a center at the same time, providing some peer-group support. Providing a good supply of employment leads--either from Job Service orders or other sources--would also improve the usefulness of the centers.

VI. TRAINING AND RELOCATION

Classroom and on-the-job training opportunities were offered to treatment 2 claimants at the assessment/counseling interview, the purpose of which was to test the efficacy of a treatment that attempts to alter or upgrade the skills of individuals whose current set of job skills are no longer in demand. Individuals in this treatment group could also choose to relocate to another area in which their skills were in demand, and, in this case, they were offered financial support for out-of-area job search and moving expenses. In addition, if they believed that jobs which did not require training were available in their area, they could pursue job-search only. The services available to this last group were discussed in the previous chapter.

In this chapter, we examine the offer of training and relocation assistance and the receipt of those services by claimants. In the first section, we discuss the mechanisms used to offer training to claimants and to place them in training. In the second section, we examine the participation of claimants in the training component and discuss the nature of their training. The third section assesses the training offer and placement process in an attempt to identify the factors that contributed to the varying success of the individual sites in terms of placing individuals in training. In the final section, we discuss the experience of claimants with relocation assistance.

A. THE TRAINING OFFER AND PLACEMENT PROCESS

Classroom and on-the-job (OJT) training opportunities were offered to claimants in treatment 2 when they were administered the assessment/ counseling interview. The job search objectives of claimants were examined in light of their past experience, test scores, and interests, and they were informed about the availability of OJT and classroom training.[1] This systematic exposure to the availability of training was expected to channel more individuals into this option than would be the case in the current service environment and in the other treatments.[2]

The outcome of this assessment interview was a decision about which option the individual wanted to pursue--classroom training, OJT, or job search with the possibility of a relocation allowance.

---

[1] Relocation assistance and local area job search were also discussed.

[2] When individuals in either treatment 1 or 3 expressed interest in training, they were referred to the local JTPA program operator rather than directly to the JTPA assessor who was assigned to the NJUIRDP. This procedure was adopted because it duplicated the process that was being followed in the current service environment.

This decision was made jointly by the staff member and claimant, with the individual's interests and desires given substantial weight in the process. Participation in classroom training or OJT was voluntary (as was relocation). In addition, the staff member was permitted to refuse to approve training (or a particular course) if he or she considered it to be unsuitable for the claimant.

In the initial design, this introduction to training, together with the assessment interview, was to be provided by a single demonstration staff member--the JTPA assessor assigned to the project. The JTPA assessor was assigned because the existing local JTPA network of training opportunities provided the primary source of both classroom training and OJT slots.[3] Thus, JTPA staff were selected to coordinate and operate the training placement process for the demonstration. However, in actuality, all but one office adopted a two-step process whereby the claimant first saw the ES counselor for an interpretation of the test results and for general JTPA certification; the ES counselor also often provided a general introduction to training opportunities. The claimant then met with the JTPA staff member for a more intensive discussion of the training opportunities, although in one office the claimant was referred to the JTPA staff member only if he or she expressed interest in training.

The use of this two-step process evolved from the initial design for two reasons. First, the JTPA staff members assigned to the demonstration had, with one exception, not been trained prior to the demonstration to interpret GATB test results. Initially it was expected that these individuals would be trained to interpret the test results for the demonstration. However, it was finally decided that the JTPA staff did not have the appropriate counseling qualifications to be trained and that only the ES counselors could interpret the test results. Second, the ES performed the JTPA certification process in most local areas in New Jersey prior to the demonstration, and ES staff needed to see the claimants anyway to determine JTPA eligibility.[4]

Once the assessment interview was completed, the course of treatment 2 differed according to the basic option that was chosen. If classroom training was chosen, the JTPA staff member attempted to arrange for training; in most sites, the staff member relied on a list of local training options and vendors used by JTPA. Three restrictions were placed on acceptable classroom training: (1) that the expected duration of courses was to be no longer than six months,[5] (2) that claimants were to be offered remedial education only if necessary to progress to a job-oriented training course, and (3) that, with the exception of remedial education, purely academic courses were not to be funded (the courses were to be job-oriented). The training was not required to be performance-based, in the belief that this requirement might limit the availability of training for some workers who in the opinion of the training institutions would not be expected to perform adequately. Nonetheless, most of the training provided by the JTPA program operators was performance-based.

---

[3] For classroom training, only approved vendors could be used. Approvals are generally made by the N.J. Departments of Higher Education and Education. The Commissioner of Labor can also approve additional programs not covered by these departments.

[4] Demonstration participants who were not JTPA-eligible could have received training that was paid for directly with demonstration funds.

Staff were instructed to make active efforts to enroll the individual in suitable training as quickly as possible following the assessment interview. Until training was arranged, however, the individual was expected to comply with the UI job search requirements and with the demonstration's periodic JSA follow-ups, described in Chapter V. Once training was arranged, the individual was excused from UI work search requirements (for up to three weeks) while waiting for the beginning of training. Once training began, participation was monitored bi-weekly by JTPA and reported to the UI district representatives, who handled the payment of UI benefits via the mail (this payment procedure is applied regularly to all JTPA trainees in New Jersey).[6] During training, the individual was also excused from work search requirements.

Once training was completed, the individual was expected to be called in by the JTPA staff member for an exit interview. As part of this interview, the staff member was to review the individual's employability plan and update it with the individual, as appropriate. The staff member was also to review the individual's job search techniques and strategies and inform him or her about the availability of the resource center.[7] If the individual did not have a job and continued to claim UI, work search was monitored in the same manner as for nontrainees. In practice, this follow-up was not always undertaken as soon as training was completed. Since the vendors were generally operating under performance-based contracts, the vendor was expected to place the trainee in a job in order to receive the full payment. In some sites, the JTPA program operators gave the vendors time to place the trainees before the operators became involved in the placement process themselves.

---

[5] The design permitted training to last longer than six months, particularly if the claimant needed remediation prior to occupational training. For cost reasons, longer durations were supposed to be restricted to at most 5 percent of the trainee population in each site; however, all but one site exceeded this restriction. Overall 16 percent of the classroom training lasted longer than 6 months with 6 percent lasting more than 7 months.

[6] In some sites, the vendors reported directly to UI on a bi-weekly basis.

[7] During the design process consideration was given to providing the job search workshop to trainees at the end of their training. However, it was decided to provide the workshop to claimants prior to any training, primarily because the marginal cost of providing the workshop initially was low, given that claimants in the other treatment groups were also being given the workshops. Providing workshops after training was expected to be more expensive, particularly if scheduled workshops were unavailable. Providing workshops after training would also have required an additional referral mechanism to send training completers to the workshop. Even without these considerations, however, providing workshops after training would have meant that anyone who initially expressed interest in training would have been excused from the initial workshop. Since not everyone who expressed interest in training entered training, a mechanism would have been required to place such individuals in workshops quickly enough to achieve the goal of early intervention. Furthermore since the workshops were required, it is likely that some claimants would have expressed an interest in training merely to avoid taking the workshop. This response, would have made it yet more difficult to achieve the goal of early intervention.

For individuals who chose OJT, the JTPA staff member attempted to enroll the individual in an OJT job. Local JTPA OJT arrangements were used if available. If no OJT jobs were immediately available from current slots, the JTPA staff member was instructed to attempt to develop additional slots for the individuals in the program, which was usually done through the local JTPA program operator staff who specialized in OJT. The demonstration design also called for encouraging the claimants themselves to find their own OJT opportunities, which, of course, were to be approved by program staff and formalized through a contract. To help the individual in this effort, the design suggested that the claimant be provided a voucher to indicate to potential employers that the claimant was eligible for an OJT subsidy. Although no OJT guidelines are currently imposed for the JTPA program, a set of guidelines and minimum standards was used for the demonstration. This set of standards was quite flexible and did not impose any unusual or highly restrictive requirements.

Until the individual entered an OJT job, he or she was expected to comply with the UI work search requirements, and he or she was also subject to the periodic demonstration monitoring of work search activities discussed in the previous chapter. Once the OJT job began, progress in the job was monitored to authorize the payment of the subsidy. At the end of the OJT period, JTPA staff determined whether the OJT employer would retain the individual as a permanent employee. If employment did not continue and the individual resumed collecting UI, periodic job-search monitoring was resumed.

A final point that should be noted about the training offer and placement process was that it was expected to vary somewhat in each local area, in part because it relied on the training opportunities developed by the JTPA program operators in the local Service Delivery Areas. These operators were also encouraged to expand these opportunities for demonstration members and were informed that demonstration funds could be used to pay for training that would not be covered under JTPA (e.g., for training that was not in the local area); it was expected that some staff would be more creative or active than others in developing expanded training opportunities, which would contribute further to the variation among the sites. Differences among sites in terms of the supply of training slots or of potential OJT opportunities (independent of the access of JTPA program operators to these slots or to prospective employers) were also expected to contribute to the variation among the sites. These expected differences in the nature of the training components of the sites differ from the differences that have been described for the other employment services. In the latter cases, the design stressed uniformity in the delivery of services; for training, the design stipulated only that the offer be made, and that a strong effort be made to encourage training and to find suitable training. The types of classroom training or OJT to be used were not described in the design, as opposed to, for instance, the job-search workshop, for which a set curriculum was established.

B. TRAINING PARTICIPATION AND THE NATURE OF THE TRAINING

Data on participation in training, reported in Table VI.1, show that 15 percent of the individuals who were assessed and offered training were enrolled in either classroom training or OJT. The vast majority (13 out of the 15 percent) were enrolled in classroom training as compared with OJT. Several observations can be made about this overall rate of participation in training.

First, the rate is higher than the training participation rate experienced by claimants in the control group, indicating that the demonstration did achieve its goal of directing more training toward the NJUIRDP-eligible population than would occur in the current service environment. This conclusion is based on data presented in the impact and benefit-cost report that show that 2.3 percent of control group members received training from JTPA. These data also show that 9.4 percent of all claimants in the JSA plus training or relocation treatment received some training.[8]

Second, although the training participation rate was higher than for the control group, it was not as high as initially expected based on experience in previous dislocated worker demonstrations and in JTPA Title III.[9] In Section C, we discuss some reasons that training participation rates were lower than expected.

Third, the relatively low rate of OJT relative to classroom training was also unexpected. Instead, it was thought that, since demonstration-eligible individuals had substantial work experience (based

---

[8] The 9.4 percent number differs from the 15 percent number presented in the previous paragraph because individuals who were not assessed are included in the base used to compute the 9.4 percent figure. Some of these individuals received training.

[9] For example, the Buffalo dislocated worker demonstration program evaluated in Corson et al. (1985) had a classroom training participation rate of 18 percent and an OJT participation rate of 26 percent. A recent GAO (1987) study of Title III reports participation rates of 26 percent for classroom training and 16 percent for OJT in a survey of 715 projects. As we discuss below, these participation rates may not be a good guide to the rates that could be expected to occur in the demonstration.

TABLE VI.1

TRAINING PARTICIPATION RATES FOR CLAIMANTS WHO WERE ASSESSED
(percent)

	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
Classroom Training	10.5	21.9	11.1	5.9	14.7	14.1	10.7	8.4	23.7	10.6	13.3
OJT	4.3	1.8	0.5	2.6	1.3	0.4	0.7	3.8	3.5	0.6	1.9
Total	14.8	23.7	11.6	8.5	16.0	14.5	11.3	12.1	27.3	11.2	15.2
Sample Size	209	224	190	153	232	227	291	239	198	170	2,133

NOTE: Individuals were counted as having received training if either the PTS or the JTPA data base indicated that they had received training after their date of claim.

on the selection criteria), OJT would provide a good avenue to reemployment. It was felt that employers would be willing to hire them because of their work experience and because the subsidy provided by OJT would pay for whatever employer or job-specific training they might need. However, this was not the case in the demonstration, and we discuss in Section C some of the reasons that it was not.

Before discussing the nature of the training that was provided, it should be noted that the training participation rates varied substantially by office. As shown in Table VI.1, the classroom training participation rate ranged from a low of 6 percent in Butler to a high of 24 percent in Burlington. Similarly, OJT participation rates ranged from under one-half of 1 percent in several offices to just over 4 percent in Paterson. These site-specific data show that two sites in particular--Hackensack and Burlington--were quite successful at promoting training. Bloomfield ranked as the third most successful site although the difference between this site and the others was not as large as for Hackensack and Burlington. Thus, in the next section, we compare how these sites handled the training offer and placement process with the approach used elsewhere, in an attempt to identify the factors that contributed to placement success.

With respect to the characteristics of the training provided in the demonstration (see Table VI.2), it took an average of about 4 weeks to authorize classroom training after the offer was made at the assessment interview and about 6 to 7 weeks to begin classroom training or OJT. The medians were lower--3 and 5-6 weeks for authorization and the start of training, respectively. In part, these averages are higher than the medians because some offices did not rely extensively on open enrollment training but on training with fixed start dates. In these cases, claimants often had fairly long waits until the fixed start date before they began training.

The expected number of weeks of training was 18.4 for classroom training and 13.9 for OJT; the OJT subsidy averaged $4.28 per hour. The mean costs were $2,723 for classroom training and $1,960 for OJT. These mean costs differ somewhat from the typical experience in JTPA Title II in New Jersey, since classroom training costs were somewhat lower than the average, and OJT costs were somewhat higher. The costs of classroom training were lower because the courses were relatively short, having been used to upgrade the claimants' skills rather than to provide training in a whole new area. The costs of OJT were higher because the claimants had substantial work experience, and the OJT slots that were obtained offered much higher hourly wages than is typical for OJT. The mean

TABLE VI.2

TRAINING CHARACTERISTICS

	Classroom Training	OJT
Mean Days from Assessment to Training Authorization Date	30.6	n.a.
Mean Days from Assessment to Training Start Date	44.2	48.2
Mean Expected Number of Weeks of Training	18.4	13.9
Mean Expected Number of Hours	430	546[a]
Mean OJT Subsidy per Hour	n.a.	$4.28[b]
Percent Performance-Based	82.3	n.a.
Percent with Transportation Allowance	10.2	n.a.
Mean Cost per Trainee[c]	$2,723	$1,960
Percent Distribution of Training by Subject		
Business and management	3.5	n.a.
Business and office	36.2	n.a.
Marketing	3.2	n.a.
Computer and information sciences	26.8	n.a.
Consumer, personal, and miscellaneous services	2.0	n.a.
Engineering	6.3	n.a.
Allied health, home economics	3.2	n.a.
Law	2.0	n.a.
Basic skills	1.6	n.a.
Construction	1.6	n.a.
Mechanics and repairers	7.5	n.a.
Transportation and material moving	1.1	n.a.
Precision production	2.4	n.a.
Other	1.6	n.a.
Not available	1.1	n.a.
Number Who Received Training	254	38

n.a. = not applicable

[a] Almost all OJT slots were 40 hours per week.

[b] The subsidy in almost all cases equalled one-half the hourly wage.

[c] These estimates were computed from data submitted to NJDOL by the Service Delivery Area program operators.

subsidy of $4.28 is based on a mean hourly wage of approximately twice that amount ($8.56). Most JTPA OJT slots in New Jersey offer wages that are nearer to $5.00 an hour.[10]

The data in Table VI.2 also show the subjects for which classroom training was provided. The two major areas were (1) business and office and (2) computer and information services, which accounted for 36 and 27 percent of the total, respectively.[11] Employment prospects are strong in New Jersey for occupations in both of these areas, as they are for a number of the other areas in which training was provided. About half of the OJT occupations (not reported in the table) were in technical, clerical, and sales occupations. Thus, it appears that the training which was offered was directed towards occupations that are in demand in New Jersey. A further point to note about the nature of the classroom training is that, as mentioned earlier, a number of the courses were designed to upgrade existing skills rather than to develop totally new ones. For example, one individual with accounting skills was trained to use a personal-computer-based spread-sheet package, a skill which should enhance this individual's suitability for jobs in today's marketplace. This type of training is somewhat at odds with past program practice, whereby it has been thought that this type of upgrading will be undertaken by employers, and that publicly funded training of this type is unnecessary.

Data not reported in the table show that a wide range of providers were used including community colleges, private colleges, vocational education schools, and private training institutes. Overall about 100 different vendors provided training to the 254 classroom training recipients.

Finally, Table VI.3 provides data on the outcomes of the training provided in the demonstration.[12] These data show that 66 percent of the classroom training participants were known to have completed training. Most of these appear to have been placed in jobs by the providers. Another 12 percent dropped out, and 5 percent left training for a job. The status of the remaining cases was reported as unknown. In terms of OJT, we find that about three-quarters of the participants appear to have remained with the employer at the end of the OJT period.

---

[10] These comparisons to New Jersey experience in the JTPA Title II programs are based on discussions with state staff.

[11] Within the business and office category, the training was concentrated in accounting, business data processing, and secretarial occupations.

[12] These data were not available for the full sample, since some individuals had not completed training at the time the data were collected.

TABLE VI.3

TRAINING OUTCOMES
(percent)

	Classroom Training	OJT
Termination Type		
Completed	65.5	n.a.
Left training for job	4.5	n.a.
Dropped out	12.4	n.a.
Unknown	17.5	n.a.
Placed by Provider	58.2	n.a.
Retained Employment	n.a.	75.8
Sample Size[a]	177	33

[a] The sample sizes are smaller than those shown in Table VI.2 because, as of the date the data were collected, 77 classroom training and 5 on-the-job training cases had not been terminated.

C. ASSESSMENT OF THE TRAINING OFFER AND PLACEMENT PROCESS

The discussion in the previous section indicates that the NJUIRDP program was successful at placing more claimants in training (classroom and OJT) than would have been the case in the regular service environment (i.e., generally through ES referrals to JTPA). The discussion also concluded that the training that was provided appeared to be appropriate, in that it was concentrated in occupations that are in demand in New Jersey and that are expected to continue to be in demand.

However, the overall training participation rate, particularly the OJT participation rate, was lower than might have been expected, based on the experience of other dislocated worker programs. The participation rate also varied by site, with three sites having been more successful than the others in placing individuals in training. Because of this mixed experience and because the participation rate was lower than anticipated, this section focuses on identifying the factors that affected the participation rate. The discussion consists of two sections. The first examines the nature of the NJUIRDP intervention and asks whether the experience with training in other dislocated worker programs provides a good guide for comparisons with the NJUIRDP experience. The second section then examines how the training offer and placement process differed by site, to determine what factors led to the different participation rates.

Before proceeding with that discussion, a third potential explanation for the lower-than-expected training participation rate should be mentioned and dismissed. That is, it is possible that training participation could have been limited by the unavailability of training opportunities. However, we found no evidence to suggest that this was the case.[13] The range of both public and private classroom training opportunities in the state is wide, and, given the geographic concentration of the state, access to training should not have been a problem for claimants. In addition, since the state economy was quite strong during the demonstration period, the demand for labor was high, which should have helped make developing OJT slots possible.

1. The NJUIRDP Intervention

Several reasons explain why training participation rates in the NJUIRDP should be lower than the rates that have been experienced in other dislocated worker programs.

First, by design, the NJUIRDP provided an early intervention. Services began at about the fifth week of unemployment, and the training was offered at about the seventh or eighth week. At that point, many individuals who might ultimately have been interested in training may have felt that they could find a job on their own, and that any assistance, let alone training, was unnecessary. Discussions with the demonstration ES counselors and JTPA staff suggest that this was often the case. Only about 40 percent of the claimants who were assessed were recorded at that time as having expressed interest in classroom training or OJT, and some counselors believed that some claimants said that they were interested only because that was the answer that was expected from them. These staff observations were confirmed in the follow-up interview where 36 percent of the claimants who were assessed said that they were interested in classroom training and 12 percent said they were interested in OJT. Three main reasons given for not being interested in classroom training or OJT were that the claimant was not interested in any training, the claimant was not interested in the type of training offered or the claimant was expecting to be reemployed. The main reason given for rejecting training among those who considered training was that they didn't want to change their career or field.

---

[13] A number of sites had developed extensive lists of the available training slots in their local areas.

A second design factor that affected participation was that the timing of the service delivery in the demonstration, as well as the mandatory nature of the initial services, differed from other programs. Some individuals who were assessed and offered training were there because they were told that they had to attend the session to collect UI, not because they were seeking any services. Such individuals would presumably be less likely to be interested in training than those who would attend voluntarily. In contrast to this situation, the Buffalo demonstration that was cited earlier offered reemployment services an average of one year after layoffs occurred, and the training participation rate of 44 percent in that demonstration pertains to the individuals who were unemployed at the time of the offer and who voluntarily attended orientation sessions. This group represented only about 20 percent of those who were contacted by the program. Thus, the experience of these other programs is probably a poor guide for explaining the training participation rates in the NJUIRDP.

A final design factor that affected participation also pertains to the early nature of the intervention. As described in Chapter III, it is difficult to identify at an early stage in the unemployment spell those who really need services (i.e., those who will be unemployed for a long time in the absence of assistance). Consequently, some NJUIRDP-eligibles clearly did not need training. In fact, because it is difficult to test job attachment, some individuals who were selected expected to be recalled (but had no definite date) and were eventually recalled by their former employers. To gain some insight into how demographic and other individual characteristics affect training participation, we estimated a simple regression model using training participation as a function of the characteristics of claimants. Our sample consisted of individuals who were assessed and then offered training. This regression showed that, as expected, the claimants who expected to be recalled had a significantly lower likelihood of accepting training. Age was also a factor; older individuals were less likely to receive training. As might also be expected, claimants with longer potential UI durations were more likely to accept training, presumably because the longer potential duration of benefits provides income support during the training period. Each of these characteristics was also cited by program staff as an important factor in the acceptance of training. Several other variables were statistically significant and should also be mentioned. Women were more likely to accept training than were men, and blacks and Hispanics were more likely to accept training than were whites. Neither of these findings is surprising given the labor market difficulties often faced by these demographic groups.

2. Implementation of the Training Offer and Placement Process

As indicated above, several sites were more successful than others in placing individuals in training, and a comparison of the approaches used in these sites with those used by the others may provide some guidance to future program operators who wish to direct training towards displaced workers. It should be noted that differences in the demographic composition of the sites do not explain the different training participation rates. When site dummy variables were included in the regressions cited previously, we found that the probability of training participation in two sites--Hackensack and Burlington--continued to be significantly higher than the probability for all other sites. Bloomfield also had a significantly higher probability than some of the other sites.

When we compare these three most successful sites with the others, we find some differences which we believe contributed to their success.[14] However, it should be noted that not all factors that we believe were important were present in all of these sites, and, in some cases, factors which we believe adversely affected the training participation rate were themselves present in one or more of these sites. Therefore, the observations listed below should be viewed with caution. They do not provide a fail-safe guide to success, nor are they necessary for success.

We believe that the following factors were important ingredients of the most successful programs:

- The successful sites introduced training early in the service sequence. Presentations were made by JTPA staff either at orientation or in the job-search workshop. By the time that claimants had reached the assessment/counseling interview, they had the time to think about training as an option.

---

[14] We are defining success in terms of the training participation rate, yet it is possible that some claimants might have been placed in inappropriate training.

- Staff in the successful sites were very enthusiastic about training and promoted it vigorously to the claimants. Claimants were told how training could complement their existing skills, and they were encouraged to enroll in training.

- The successful sites used flexible training arrangements more than some other sites. That is, they placed claimants in individualized training slots that started at any time, rather than relying primarily on vendors whose classes began at fixed calendar times.

- The successful sites were generally able to seek out new training opportunities and to direct claimants to them without an extensive bidding and contracting process. In contrast, some other sites had quite rigid contracting requirements that made it difficult for the JTPA staff to rely on a wide range of training opportunities.[15]

- To be successful, the JTPA staff who were assigned to the project had to have access to other JTPA staff to handle any contracting issues and, in general, to develop any OJT slots. This access was facilitated when project staff were experienced or when they were aggressive enough to develop the necessary contacts.

Three additional implementation issues should also be mentioned. First, the decision to rely on the JTPA system to provide training referrals was based on the fact that the system already performed this function. However, most of the local JTPA program operators had actually had relatively limited experience in providing classroom training and OJT to displaced workers, as opposed to the disadvantaged population also served by JTPA. This limited experience is illustrated in Table VI.4, which shows the distribution of services provided to JTPA Title III enrollees in Program Year 85, which was the year prior to the start of the demonstration. The data show that most of the Title III enrollees in all sites in that year received job search workshops rather than training. In no sites was OJT a major service activity, and in only two sites (Hackensack and Elizabeth) was classroom training provided to a significant proportion of the Title III population. However, even in these sites, the actual number of trainees was not large--30 to 40 individuals. Thus, the JTPA program operators often had to adjust their operating procedures and approaches to deal with the type of workers who were eligible for the NJUIRDP. This process took time and probably contributed to the overall lower-than-anticipated training participation rate in this period.

Second, as mentioned earlier, OJT was utilized infrequently in most sites, in part because available OJT slots were generally geared toward lower-wage jobs than were appropriate for the NJUIRDP population. New slots had to be developed, which in most sites was undertaken by the regular JTPA job development staff rather than by the NJUIRDP JTPA staff member. This necessary linkage was not always developed.

---

[15] Interestingly, the Bloomfield site was initially restricted to using a small set of vendors with fixed training schedules. When this situation, which restricted initial placements, was changed, the program became one of the most successful.

TABLE VI.4

DISTRIBUTION OF SERVICES PROVIDED TO JTPA TITLE III ENROLLEES, BY SDA, PROGRAM YEAR 85

Site/SDA	Job Search Workshop	OJT	Classroom Training	Holding	Title III Enrollees
Paterson/Passaic	90.1	0.0	1.6	8.3	444
Hackensack/Bergen	88.6	0.3	10.0	1.2	341
Jersey City/Jersey City	92.3	0.0	2.7	5.0	339
Butler/Morris	88.7	1.0	1.7[a]	8.7	599
Bloomfield/Essex	94.2	0.0	0.7	5.2	291
Newark/Newark	88.0	0.0	6.0	6.0	300
Elizabeth/Union	84.0	0.8	10.5	4.8	400
Perth Amboy/Middlesex	88.0	0.7	4.1	7.2	292
Burlington/Burlington	96.4	0.6	1.4	1.7	362
Deptford/Gloucester	89.0	1.1	7.3	2.6	191

SOURCE: Special tabulations provided by the NJDOL, Employment and Training Division.

[a] Includes 4 individuals in occupational training and 6 in English as a second language training.

Finally, there was a point near the end of the demonstration period at which it was uncertain whether or not sufficient funds were available to place individuals in training. Because the initially allocated funds had been used, additional funding sources had to be found. Claimants who desired training were put on hold until this issue was resolved. There is some evidence that this situation did affect training participation. Several claimants who were interviewed and said that they were interested in training also said that they were told that funds for training were unavailable. In addition, when month of selection is controlled for the regressions mentioned earlier, we find that claimants who were selected in the last 3 months had a lower training rate (about 3 percentage points) than did claimants who were selected earlier. If we assume that this holding period accounted for this lower rate, we can calculate that if the holding period had not occurred, the training participation rate might have been about 16 percent. Of course other factors, such as the beginning of the phase out period, may have also depressed the training participation rate in this period.

D. RELOCATION ASSISTANCE

Treatment group 2 claimants who were assessed received, in addition to the offer of training, an offer of relocation assistance to help them, if they were interested, find an out-of-area job and if they accepted a job, move to the area. More specifically, the potential relocation assistance consisted of two components: (1) payments for out-of-area job search if job interviews were prearranged and (2) payments for moving expenses. Payments for job search were based on expenses up to a maximum of $400. Multiple trips were permitted, and trips were to exceed 50 miles one way. Relocation payments were paid only when a job was available and the move exceeded 50 miles. The payment was a fixed amount that ranged from $300 to $1,000, depending on the distance moved.

In previous demonstrations (see, for example, Kulik et al., 1984, and Corson et al., 1984), it was found that relatively few individuals took advantage of relocation assistance, which was also expected to be the case in the NJUIRDP.[10] In fact, because of the early intervention of the NJUIRDP, participation rates in the NJ demonstration could be expected to be even lower than in previous demonstrations, since laid-off individuals are likely to exhaust job possibilities in local areas before considering out-of-area possibilities.

---

[10] The take-up rate for moving assistance ranged from 0.2 percent to 9.1 percent in the dislocated worker demonstrations examined in Corson et al. (1984). The lower rates were found in the more urbanized sites, and the higher rates (5 to 9 percent) were found in two rural sites. The site with the highest rate also contained a unique dislocated worker population (construction workers). The rates in the more urban sites provide a better comparison with the NJUIRDP experience.

As expected, few individuals accepted relocation assistance. About one percent of the treatment group 2 claimants who were assessed were recorded by the counselors as interested primarily in relocation assistance.[17] About half (10) of these individuals were recorded in the tracking system as having received some relocation assistance. Another six individuals also appear to have received assistance, based on data from the payments system. Regardless of which data base is used, the participation rate for relocation assistance was quite low (0.5 to 0.8 percent of those who were offered this service).

All but one person who accepted such assistance received assistance for out-of-area job-search trips, with most recipients having taken multiple trips. The average cost of the trips was $50. About two-thirds of the trips were out-of-state, and most exceeded 100 miles. In all cases, the trips were recorded as single-job-interview trips rather than as multiple ones. Four individuals also received moving assistance; two received $300 each, and two received $1,000.

These low levels of relocation assistance receipt were expected, and, as stated earlier, the counselors indicated that few individuals were interested in moving. They also indicated that the few who were interested in moving tended to be younger and from white-collar occupations. This view is confirmed partially by the data.[18] The average earnings of the individuals who accepted relocation assistance were quite high prior to UI[19] (about $40,000 a year), suggesting that this group had well-paying white-collar jobs. However, these individuals were not particularly young--7 out of the 10 were 45 to 54 years old. A final point that should be noted about relocation assistance is that four offices (Hackensack, Bloomfield, Perth Amboy, and Burlington) accounted for all but two of the payments. With the exception of Perth Amboy, these offices were generally located in higher-income areas than was true of the other offices, which again fits the view that higher-income claimants were more likely to accept this type of assistance.

---

[17] In our site visits, counselors confirmed that most individuals were not interested in relocating. The interviews also provided confirmation of this observation. Eighty-four percent of the claimants who said that they were not interested in relocation assistance said that they didn't want to relocate.

[18] Interestingly, all of the individuals who received relocation assistance were men.

[19] This statement is based on an examination of the base year earnings used to determine UI eligibility and benefits.

VII. THE REEMPLOYMENT BONUS

The third treatment package included a reemployment bonus that was offered to claimants at the assessment/counseling interview. The purpose of the reemployment bonus was to provide direct financial encouragement for displaced workers to seek work actively and become reemployed, thus mitigating the work disincentives inherent in the UI benefit structure. This purpose was based on the premise that, while many displaced workers have marketable skills, they may lack the motivation to seek reemployment rapidly or may have unrealistic job goals. Accordingly, this treatment was designed to simulate a UI benefit cash-out program to the extent possible, whereby claimants receive at least part of their remaining entitlement as a reward for not exhausting it. Cash-out programs have been proposed as a possible way to restructure unemployment insurance programs (see USDOL, 1986, for a review). A reemployment bonus has also been tested in Illinois with positive results (see Woodbury and Spiegelman, 1987), and it is currently being tested in two additional demonstrations in Pennsylvania and Washington.

In this chapter, we examine the experience in the demonstration with the reemployment bonus. We begin in the first section by describing the bonus and some of the reasons that it was structured as it was. We then describe the payments mechanism used in the demonstration to pay the bonus. In the final section, we discuss the rate of bonus receipt and the timing and amount of the bonus.

A. THE REEMPLOYMENT BONUS OFFER

The design of the NJUIRDP recognized that the different goals underlying the bonus would require different bonus schemes to create appropriate incentives for the claimants. A decision was reached that the main goal was to promote rapid reemployment, and that this goal could best be achieved by providing a large benefit cash-out bonus to those who secure a new job quickly, and providing smaller bonuses to those who take longer to become reemployed. A variant of this goal was considered whereby rapid reemployment would be promoted by encouraging claimants to accept reduced wages more quickly. This variant could be accomplished by tying the bonus to the difference between the wages of the post-unemployment and preemployment jobs. While this scheme would have operated directly on one of the major factors that might inhibit rapid reemployment (i.e., the acceptance of realistic market wages by claimants), its implementation would have entailed a cumbersome benefit determination process.

It was possible that additional types of behavior to be modified could have been identified. However, a major problem emerges with more complicated reemployment bonus schemes: as the schemes become more complex and fine-tuned to promote specific types of behavior, the nature of the incentives becomes less and less comprehensible to the average claimant, diminishing the overall value of reemployment bonuses. The degree to which this happens is an empirical question, but, nonetheless, testing many variations was beyond the resources of this demonstration. Instead, it was decided that the NJUIRDP should test a strong, uncomplicated, and easily understandable treatment to examine the most basic question associated with the ability of a reemployment bonus to improve the timing of reemployment. Thus, the treatment entailed providing a large incentive for early reemployment, and fading out the incentive over time until it fell to zero at an appropriate point.

The specific offer that was made to eligible claimants during the assessment/counseling interview was one-half of the remaining UI entitlement if he or she started work by the end of the second full week following the interview.[1] This full bonus averaged $1,644, but was considerably higher for some claimants than for others (the maximum was $2,394). The bonus then declined by 10 percent of the original amount each week, so that it fell to zero by the end of the eleventh full week of the bonus offer (or it expired at the end of the UI entitlement period, whichever came first).[2] Claimants were provided with information on the specific bonus that was offered to them, and they were given a fact sheet that described the bonus scheme. Individuals who were offered the bonus had access to the resource center, and their job-search activities were subject to the same monitoring procedures as were described in Chapter V.

When an individual found a job, he or she claimed the reemployment bonus by reporting the new job to his or her ES counselor. The Employment Service was responsible for verifying employment by calling the employer. To qualify for a reemployment bonus, the claimant's new job must not have been temporary, seasonal, part-time (under 32 hours per week), provided by a relative, or provided by the immediately preceding employer. These characteristics (or the lack thereof) were established when the verifying telephone call was made. These were the only "suitability" criteria that were applied.[3]

---

[1] For example, if the interview was held on the Tuesday following the job-search workshop, the claimant had two-and-one-half weeks (the rest of the current week and the following two weeks) to start working in order to receive the full initial bonus amount.

[2] Although the reemployment bonus was available for eleven full weeks, the period of decline in the bonus was a ten-week period from week 2 to week 11.

A job tenure requirement was also attached to the bonus payment. An individual had to be employed for four weeks to receive the initial payment of 60 percent of the bonus amount, and he or she had to remain employed for another eight weeks to earn the final 40 percent. Thus, the counselor verified job-holding at each of these points (four and twelve weeks after the start of employment).[4],[5]

The bonus was calculated through the Participant Tracking System from the date on which the job actually began, not the date on which the job was secured. UI payments continued until the job began. For most participants, a week of UI payments was about the same as an additional week of bonus payment.

This scheme provided major incentives for early reemployment. The major advantage was that it was simple and direct. Claimants could relate to a large bonus that was tied to UI benefit entitlement and to a simple schedule of declining benefits. However, since the bonus offer was not something that claimants expected and were familiar with, an important issue is whether or not claimants believed and understood the offer. Evidence from the interview suggests that most claimants probably understood the offer. However 22 percent said that they didn't receive the offer or didn't remember it at the time of the interview.

B. THE PAYMENTS MECHANISM[6]

An important concern in designing the reemployment bonus for the demonstration was that a system be developed that would provide sufficient accounting controls for bonus payments while providing payments to claimants in a timely manner. The system that was adopted achieved these objectives through a three-part process, whereby (1) claimants requested a bonus payment in writing, (2) the local office staff verified eligibility and authorized the payment amount, and (3) central office staff made the payment. More specifically, the procedures were as follows.

---

[3] Additional criteria, such as whether the job was commensurate with the individual's skill level, were discussed during the design phase. It was decided, however, not to impose such additional criteria but to examine the characteristics of the jobs in the analysis.

[4] If the claimant switched jobs before the 4-week point, the start date of the second job was used to determine whether the claimant met the job-tenure requirement. If the claimant switched jobs after the 4-week point but before the 12-week point, he or she did not receive a second payment.

[5] In an ongoing program receipt of the bonus would probably preclude the collection of any additional UI benefits remaining in the claimant's entitlement. Since the NJUIRDP was a demonstration, this restriction could not be applied. The degree to which bonus recipients received further UI payments is examined in the impact and benefit-cost report.

[6] The payments system discussed herein was used for relocation assistance, out-of-area job search, and transportation allowances for classroom training, as well as for bonus payments. The procedures followed were identical.

Claimants who became reemployed applied for the reemployment bonus by sending a form (Exhibit VII.1) to the ES counselor in their local office. This claim form provided both the information that the counselor needed to confirm eligibility and a space for the claimant's signature. A signature was considered necessary in case any question of potential fraud arose; because the files would thus contain the claimant's signature requesting the bonus, the claimant could be prosecuted for fraud, if necessary, or any overpayment could be recovered through the UI system's regular recovery process. A separate claim was filed for both the 4-week and 12-week bonus payment.[7]

In the second step of the process, ES staff confirmed eligibility by calling the employer. This telephone call determined the job start date, whether the claimant was still working at the 4-week (or 12-week) point, and whether the other eligibility requirements were met. The PTS was programmed to use the job start date and the most recent UI claims data to compute the bonus payment. Staff then filled out a form (Exhibit VII.2) to authorize the payment and the amount, and sent the form to the central office payments unit. The Division of Unemployment and Disability Insurance, Collateral Claims section, was the payments unit.

In retrospect, this verification process might have been strengthened by requiring that the employer verify employment in writing rather than by telephone. As was done in the Illinois reemployment bonus demonstration, the claimant could have submitted a form to the employer to be sent to the office, or the office could have sent a form to the employer for confirmation. As we discuss below, one office actually used this latter process, and this extra step appears to have lowered the bonus receipt rate, as might be expected.

The central payments unit--the Collateral Claims section--processed the claim once it was received. This unit was chosen for this procedure to ensure that the claims would be paid quickly, since the unit was responsible for processing special UI payments (e.g., Trade Readjustment Assistance payments) and had a process in place whereby payments could in fact be made rapidly. Under this process, Collateral Claims initially verified that the payment authorization was signed by an appropriate staff member (a list of signatures of the persons who were authorized to sign payment vouchers was maintained for

---

[7] The 12-week form differed slightly from the 4-week form shown in Exhibit VII.1.

EXHIBIT VII. 1

Claim for Reemployment Bonus

Claimant Name ______________________ SS#______________

Date of Claim ______________________ LO# ______________

I am claiming a reemployment bonus under the provisions of the New Jersey Unemployment Insurance Reemployment Demonstration Project.

I began work on ______________________
Month Day Year

Employer Name ______________________

Employer Address ______________________
____
____

Name of Immediate Supervisor ______________________

Employer Telephone Number ______________________
Area Code

Hourly Wage or Gross Weekly Wage $______________ per week (circle one)
per hour

Hours worked per week __________

Claimant Signature ______________________ Date __________

EXHIBIT VII.2

NJ UI REEMPLOYMENT DEMONSTRATION
PAYMENT VOUCHER

SS# ______________ PC __________ DOC ____________

LO# ______________

PAYEE: NAME ____

First Middle Initial Last

ADDRESS ____

Number or Box No. Street

____

City State Zip Code

AMOUNT: |__|__|__|__| . |__|__|

REASON FOR PAYMENT |__|

1. First Reemployment Bonus
2. Second Reemployment Bonus
3. Transportation Allowance
4. Out-of-Area Job Search
5. Moving Expense

DATE |__|__| |__|__| |__|__|
MO DAY YR

____
L.O. Authorization

For Central Office Use

Date ______________ ______________________
C.O. Authorization

the project) and checked the amounts to determine whether they appeared to be appropriate.[8] Any questions were resolved with the local office. The checks were then mailed to the claimants. This entire process took about one week, and thus the goal of providing timely payments was met.[9]

C. RECEIPT OF THE BONUS

Data on the rate of bonus receipt are reported in Table VII.1 by office. As shown in the table, about 19 percent of the individuals who were assessed and were offered the bonus received a 4-week payment. Sixteen percent received a 12-week payment.[10] The bonus receipt rate varied somewhat by site, ranging (for the 4-week payment) from a low of 12 percent in Hackensack to a high of 29 percent in Burlington. We investigated these differences among sites by using a simple regression model to control for the characteristics of claimants. We used the individuals who were offered the bonus as the sample. The model showed that certain types of claimants who were concentrated in particular sites--namely, those who expected to be recalled and Hispanics--exhibited lower rates of bonus receipt than did other claimants. Individuals who were recalled by their former employer were not eligible for the bonus. Hispanics probably exhibited a lower reemployment rate than whites or blacks, but they may also not have understood the nature of the bonus offer as well as other claimants because of language difficulties.[11] Age was also negatively correlated with bonus receipt, which is probably correlated with reemployment. Finally, individuals with longer potential UI durations showed a higher probability of receipt. This may be due to the fact that these individuals had more time in which to collect the bonus, since the bonus offer was equal to the lesser of UI duration or the standard 11-week bonus offer period.

However, after controlling for the characteristics of claimants, we still found that claimants in Hackensack showed a significantly lower bonus receipt rate than claimants from the other sites,

---

[8] Collateral Claims staff could and did check the UI files of a claimant to determine whether the 4-week bonus amount was correct. Twelve-week payments equalled two-thirds of the 4-week amount.

[9] Since the bonus payments were taxable, Collateral Claims also mailed 1099s to recipients at the end of each calendar year.

[10] These rates are similar to the bonus receipt rate in the Illinois demonstration (Woodbury and Spiegelman, 1987), which was about 14 percent. The Illinois experiment offered a lower bonus amount ($500) than did the NJUIRDP and made the payment in a single installment.

[11] In one site, claimants with language problems were asked to have someone who spoke English call the office so that the bonus could be explained. When possible, Spanish-speaking staff were also used to explain the bonus.

TABLE VII.1

REEMPLOYMENT BONUS RECEIPT RATES, BY OFFICE FOR THOSE OFFERED THE BONUS
(percent)

	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
Percent Receiving 4-Week Payment	14.6	12.4	16.1	22.7	18.9	13.3	20.1	22.2	28.5	17.1	18.5
Percent Receiving 12-Week Payment	12.3	11.0	13.4	21.7	16.5	10.0	15.2	20.0	26.3	9.8	15.5
Sample Size	130	145	112	97	164	150	184	135	137	123	1,377

and claimants in Burlington showed a higher rate. The differences among the other sites were not statistically significant. The economies in both Hackensack and Burlington were strong, which helps explain the higher rate in Burlington but obviously not the lower rate in Hackensack. Our site visits did suggest, however, why the bonus receipt rate was low in Hackensack. The counselor in Hackensack was concerned that the telephone follow-up procedure used in the demonstration to confirm reemployment with the employer and hence to authorize the bonus was insufficient to detect potential fraud. Therefore, he sent a form to the employer to request confirmation of reemployment, and did not authorize a bonus payment until he received the written confirmation. A similar procedure was used in the Illinois reemployment bonus demonstration, whereby the claimant who requested the bonus had his or her employer send a form to the UI office for confirmation of employment.

Table VII.2 shows data on the bonus offer application and payment. The full bonus offered averaged $1,644, but the mean expected bonus was lower--$1,242 for those who applied for the bonus--because not all individuals became employed while the maximum bonus was available. As shown in the table, about 30 percent of those who received the bonus received the maximum, and about three-quarters received over 60 percent. The data also show that 87 percent of the claimants who applied received a 4-week bonus, and 73 percent received a 12-week bonus. These payments averaged $765 for the 4-week payment and $526 for the 12-week payment. Most of the individuals who applied but did not receive a bonus were no longer employed at the 4-week or 12-week point. On average, the job for which the bonus was claimed began about five weeks after the assessment interview, and payments were authorized about 4 weeks after that point, indicating that follow-up to authorize payments occurred quite promptly on average. Since it took about one week to process payments centrally once they were authorized by the sites, most claimants probably received their check within two to three weeks after the 4-week or 12-week eligibility point. Thus, the goal of making payments in a timely manner appears to have been achieved.

A final issue about the bonus offer process pertains to whether or not claimants who were eligible appear to have applied for the bonus. This issue is addressed in the impact and benefit-cost report and the conclusion reached in that analysis is that most individuals who became reemployed within the bonus period either applied for the bonus or were not eligible for the bonus, although some individuals may also have not been offered the bonus or may not have understood the offer.[12] The main reason for not being eligible was that the claimant had been recalled by the pre-UI employer.

---

[12] About 22 percent of those in the reemployment bonus treatment who were assessed said that they were not offered the bonus or didn't know what the interviewer was talking about.

TABLE VII.2

CHARACTERISTICS OF THE BONUS OFFER, APPLICATIONS, AND PAYMENTS

Characteristic	
Mean Full Bonus Offer	$1,644
Characteristics of Bonus Applications:	
Mean expected bonus	$1,242
Mean hourly wage	$9.32
Mean weeks from assessment to expected job start date	4.7
Characteristics of Payments	
Percent of applicants with 4-week payment	87.0
Percent of applicants with 12-week payment	72.7
Mean 4-week payment	$765
Mean 12-week payment	$526
Total	$1,291
Percent Receiving:[a]	
Maximum bonus	31.1
80 or 90% of maximum	23.6
60 or 70% of maximum	18.9
40 or 50% of maximum	11.8
20 or 30% of maximum	9.1
10% of maximum	5.5
Mean Weeks from Assessment to 4-Week Payment Authorization	10.5
Sample Size	
Application	293
Bonus Payment	255

[a] This distribution is based on the 4-week payment.

VIII. MONITORING COMPLIANCE

An important element of the demonstration was the mandatory reporting by claimants for both the initial sequence of services--orientation, testing, the job-search workshop, and the assessment/counseling interview--and the job search assistance follow-ups; the failure of claimants to report for services could have led to the denial of UI benefits. Compliance with these reporting requirements was monitored by the ES, which reported noncompliance to UI. UI staff then contacted the claimants, and, if appropriate, claimants were denied UI benefits until they complied with the demonstration reporting requirements.

In this chapter we discuss and assess the monitoring and enforcement process. We begin by describing the process more fully. We then report data on the degree to which the process led to UI benefit denials and the degree to which claimants complied. The final section provides a brief assessment of this process.

A. THE MONITORING AND COMPLIANCE PROCESS

As stated earlier, an important objective of the demonstration was to provide reemployment services to claimants early in their unemployment spells. This goal was to be achieved both by identifying eligible claimants and offering them services early in their unemployment spells and by compelling them, to some extent, to participate in the services. New Jersey's UI law permitted the Director of UI to require that claimants report to ES for services, but not that they participate in services, and for this reason a reporting requirement was instituted for the demonstration, as shown in the attached UI policy statement (Exhibit VIII.1). Although claimants could technically satisfy this requirement by reporting for services and then leaving, in practice most claimants who reported participated in the services.

These reporting requirements were instituted somewhat differently for the different services For the initial orientation, the notification letter requested that claimants report for orientation, and it informed them that "failure to report may affect your eligibility for unemployment benefits." At orientation, claimants were given appointment slips to report for testing and the job search workshop, unless they were explicitly excused. Later, during the workshop, they were given an appointment to report for assessment/counseling. Following the assessment/counseling interview, claimants who did not report for the follow-ups were also to be given appointments. These appointments

EXHIBIT VIII.1

POLICY STATEMENT OF ADJUDICATION
OF THE
UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT

This statement is intended to clarify the Division's policy as regards the UIRDP in instituting the mandatory reporting provision called for in the Design documents for the set of common activities including the referral to a Resource Center. These common activities are intended to intervene early on in the claimants spell of unemployment and thereby improve employment outcomes.

NJAC 12:17-2.1 (b) provides:

"A claimant will be required to report in person to the local employment service office as directed by the Division.

1. A claimant's failure to report to the local employment service office without good cause on the date and time designated will result in the loss of unemployment benefit rights from the date of the failure to report occurred, to such time as the claimant reports to either the local employment service office or the unemployment insurance claims office and is rescheduled for employment services."

The regulation clearly states that claimants are required to report "as directed by the Division" to the local employment service office for employment services. In the Project design, the initial set of common services i.e. - Orientation, Testing; Job Search Assistance Workshop; Counseling/Assessment- will be mandatory in that claimants who fail to report to the ES as directed will have established a nonmonetary issue if the claimant continues to claim UI benefits.

It is also important that appointment for each set of common services be identified by a referral/appointment slip that shows date, time, place and the Director's name for the services that occur within the ES in order to support the mandatory requirement provision of the Design.

for testing, the workshop, and the other services were provided in writing, using the form shown in Exhibit VIII.2. This form contained the date, time, and place of the appointment and the UI Director's signature to make it clear that UI had directed the claimant to report for services at an explicit time and place.

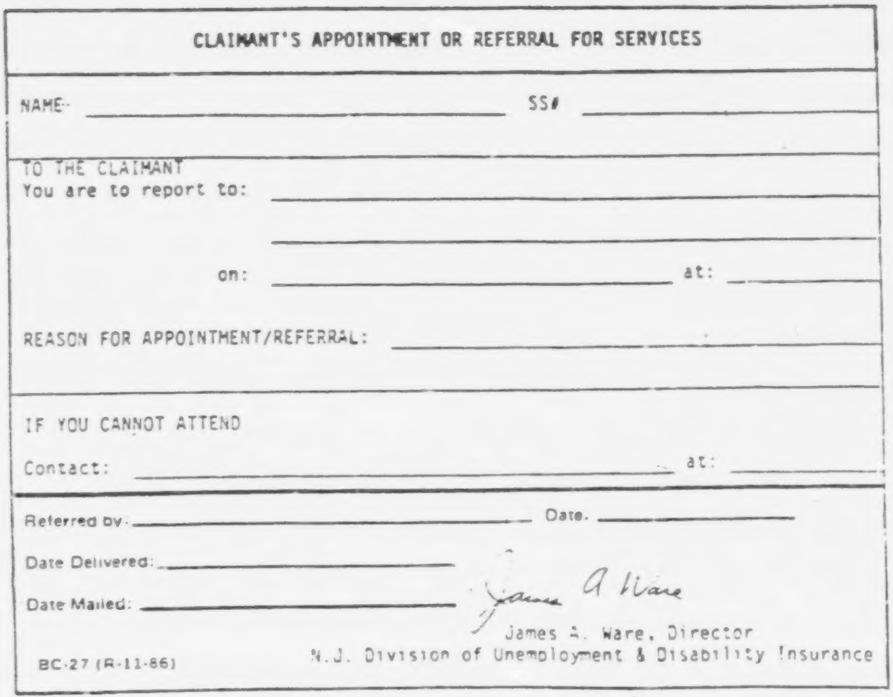
Compliance with these reporting requirements was also monitored. Attendance was recorded by ES staff in the Participant Tracking System, and a weekly Delinquency Report was produced which listed all claimants who failed to attend or to complete one of the initial scheduled events. This report was organized by event (i.e., orientation, testing, the job search workshop, and assessment/counseling). The report was sent to the local UI office. The periodic follow-up visits that occurred subsequent to assessment were not monitored in the Delinquency Report. Instead, staff were instructed to report any potential eligibility issues to UI on an individual basis, using the standard ES-572 report form that is currently used in New Jersey for this purpose (see Exhibit V.1 and the discussion in Chapter V).

The Delinquency Reports were delivered by hand to the UI claims examiner in each UI office, and that individual was instructed to pend the automated UI files for all individuals who were listed in the Delinquency Report. Any special issues or circumstances were also entered in the file. If these individuals reported to the local UI office to claim benefits, the pend indicator would not permit a payment to be made, and a fact-finding interview was triggered. The fact-finding interview and, if appropriate, a determination of eligibility were performed following NJ UI laws and regulations. The outcome of the eligibility determination depended, of course, on the reasons given during the fact-finding interview for failing to report for demonstration services, as well as on whether any other eligibility issues (e.g., availability) came to light in the interview.[1] As indicated earlier, the claims examiners were informed that failure to report to the demonstration office without good cause could lead to a UI benefit denial until the claimant reported and was rescheduled for services. They were also informed that whether the claimant received the notice to report was to be established. Consequently, individuals who did not report for orientation were not denied benefits if they said that they did not receive the letter which informed them to report for services. Instead, they were rescheduled

---

[1] Since the eligibility issues in these determinations are not related to monetary eligibility requirements nor to the initial job separation requirements, they are called "nonseparation, nonmonetary" determinations.

EXHIBIT VIII.2



CLAIMANT'S APPOINTMENT OR REFERRAL FOR SERVICES

NAME: ______ SS# ______

TO THE CLAIMANT
You are to report to: ______

on: ______ at: ______

REASON FOR APPOINTMENT/REFERRAL: ______

IF YOU CANNOT ATTEND
Contact: ______ at: ______

Referred by: ______ Date: ______
Date Delivered: ______
Date Mailed: ______

James A Ware
James A. Ware, Director
N.J. Division of Unemployment & Disability Insurance

BC-27 (R-11-86)

for services and were handed a copy of the letter.[2] If they did not report at that point, it had been clearly established that they were notified, and a denial could then be issued. The written notice that had been given to claimants by ES staff to report for the other mandatory services (i.e., testing, the workshop, and assessment) established that notice had been given in these cases.

This monitoring and compliance process, particularly the use of the Delinquency Report, was both complicated and fairly messy. Its success required a high degree of coordination and communication between the UI claims examiners and the ES staff, which, given the complicated nature of the process, generally took some time to iron out. The process itself and the rules that were established also evolved somewhat during the demonstration as problems with the process were identified. The following were the major issues and problems that arose during the demonstration:

- To be useful, the Delinquency Reports had to contain accurate data and list only those claimants who had not reported for services. This was a problem initially in some sites because ES staff did not always enter information on service receipt into the tracking system in a timely way. In those cases, claimants who actually attended services were listed in the Delinquency Report, and, consequently, some claims examiners felt that the reports were useless, and the process of pending files was not always followed.

- Even when data on service receipt were entered accurately and in a timely manner, the Delinquency Reports were not automatically "accurate." Special situations (e.g., when a claimant had called ES with a scheduling conflict and had been told to come the next week for orientation) were not handled automatically in the Delinquency Report, and required ES staff to annotate the reports before sending them to UI. In addition, UI staff often received calls from claimants directly, since the name and telephone number of the UI claims examiner for the relevant local office were included in the initial notification letter sent to the claimants. In these cases, the claims examiners made a decision about whether the claimant should report as scheduled or report for a later orientation. These special situations made it imperative that UI and ES staff develop a good working relationship to make the process work smoothly.

- The Delinquency Reports listed all individuals who had ever missed a service, not just those who missed the most recently scheduled service. Thus, as time progressed, the reports became unwieldy, since many cases had had their files pended and had never claimed additional benefits. This situation was handled in an ad hoc way by periodically deleting old cases from the Delinquency Report, but it would have been better to do so automatically. The report was also changed early in the demonstration to group claimants by the date of the missed event, so that claims examiners could easily identify claimants new to the list.

- Another change in the report was also made during the demonstration to pend the files in a timely manner when orientation was missed. Initially, the Delinquency Report for all the initial services was generated each Monday morning after the previous week's activities had been completed, together with the entry of the data on the services received. Since the orientation sessions occurred on Monday, Tuesday,

---

[2] Claimants who had a lag of more than five weeks from the missed event to the time at which they were seen by the claims examiner were not rescheduled. The purpose of this rule was to preclude dealing with individuals who had dropped out of the UI system and had not claimed benefits for a substantial period of time. It was also designed to ensure that services were only provided early in an individual's unemployment spell.

> or Wednesday, this schedule meant that claimants' files were not pended until about a week after they missed their orientation session. Thus, given the bi-weekly UI reporting process, some claimants who had missed orientation might not have been sent to a later orientation for several weeks. For this reason, the Delinquency Report was divided into two parts, and the orientation session part was generated as soon as possible after each week's orientation was completed. The other section continued to be generated after the week's activities were completed. This change was made in January 1987.

In addition to monitoring compliance through the Delinquency Report, a further report, the Case Exceptions Report, was generated centrally from the tracking system, listing all claimants who had not received an initial service and who were claiming UI five or more weeks later. These reports were generated weekly and were given to UI staff, who then sent them to the local offices to have the claims examiners check on why these claimants were continuing to collect benefits. This process was not instituted at the start of the demonstration, but was initiated in the fall of 1986. It was formalized beginning in November 1986, when the UI central office instituted a requirement whereby each UI office was to submit a monthly report on NJUIRDP nonmonetary activities to the central office. This report was to list all cases which were included in the Delinquency Reports or the Case Exceptions Reports and their disposition--the date pended, the service for which they did not report, any UI eligibility issues identified, and the date and outcome of the eligibility determination. Central office staff used this report to monitor compliance activities in the local offices.

This review of the monitoring and compliance process suggests two general points. First, the process itself was complex, requiring substantial coordination between UI and ES to keep track of the individuals who did not comply with the reporting requirements. Second, the process changed over time, becoming more focused in general and subject to more enhanced monitoring by the central office. Both points suggest that the monitoring and compliance process improved over time during the demonstration.

B. EVIDENCE ON THE DEGREE OF MONITORING AND COMPLIANCE

In this section, we examine two types of measures that provide some evidence about the degree to which the monitoring activities that were instituted in the demonstration had an impact. The first measure is the probability of nonseparation issues, nonmonetary determinations, and denials. When claimants did not report for services as instructed, an eligibility determination should have been performed if they claimed additional UI benefits, and some of these determinations should have led to the denial of benefits. Thus, it was expected that these procedures would enhance the probability of both determinations and denials for treatment group members relative to their probability for control group members.

Data on the probability of these determinations and denials for nonseparation, nonmonetary issues are reported in Table VIII.1 for treatment and control group members in each office. These data show that, overall, the probability that an eligibility determination occurred increased by 12 percentage points for treatment group members relative to controls (a difference that is statistically significant). This increase varied somewhat by office, having been lowest in Bloomfield and Deptford and highest in Jersey City and Newark. Nevertheless, the rate did increase in all offices, and all the increases except for Deptford were statistically significant (for a one-tail test at the 95 percent confidence level). The probability of a denial also increased (by about 4 percentage points), which was also statistically significant. The denial rate also varied among offices. This increase in the probability of a denial was not as large as the increase in the determination probability because the probability that a determination led to a denial of benefits was lower for treatments than for controls. Presumably, the additional determinations that arose because of the demonstration reporting requirements yielded fewer denials than did a typical nonmonetary determination.

Further insight into the nonmonetary determination process in the demonstration can be obtained from data on the issues that are adjudicated. These data, which were examined but not reported in the tables,[3] clearly show that the primary difference between treatment and control determinations was that determinations for treatment group members were more likely to involve reporting-requirement issues than were determinations for controls. About 25 to 27 percent of the issues adjudicated for treatments were reporting issues, compared with 13 percent for controls. The distribution of nonseparation issues among the other major categories was similar for treatments and controls.

The data also show why the additional determinations performed for treatments relative to controls did not increase the overall denial rate to the same extent as was the case with the determination rate. First, it was less likely that more than one issue was considered for treatments than for controls, thus reducing the probability of a denial. And, second, data on the probability that a consideration of an issue led to a denial show that the probability was relatively low for the reporting-requirement issues, which were the issues that raised the treatment determination rate.

---

[3] See the impact and benefit-cost report for a further analysis of this issue.

TABLE VIII.1

THE PROBABILITY OF A NONSEPARATION ISSUE, NONMONETARY DETERMINATION, OR DENIAL BY TREATMENT AND CONTROL GROUP STATUS

(Percent)

	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
Determinations											
Treatments	40.2	47.0	38.5	46.1	43.3	41.2	37.3	37.2	40.1	29.6	40.0
Controls	25.8	34.5	20.8	34.6	36.9	23.6	22.9	27.8	31.8	25.6	28.1
Difference	14.4	12.5	17.7	11.5	6.4	17.6	14.4	9.44	8.3	4.0	11.9
Denials											
Treatments	16.1	19.8	14.9	21.7	14.1	23.9	18.2	19.5	22.2	14.3	18.5
Controls	13.3	18.7	10.8	14.2	13.7	15.3	13.3	15.8	15.0	14.2	14.5
Difference	2.8	1.1	4.1	7.5	4.0	8.6	4.9	2.7	7.2	0.1	4.0
Sample Size											
Treatments	816	919	853	581	877	1,049	1,081	939	789	771	8,675
Controls	225	252	231	162	241	288	302	259	214	211	2,385

NOTE: The analysis was restricted to the first four nonmonetary determinations for any claimant during the current benefit year.

In summary, the data on nonmonetary determinations and denials show that the treatments raised both the probability that a determination was performed and the probability that a denial occurred, and that these increases occurred in most offices. The probability of a denial, however, did not increase to the same degree as did the probability of a determination. The additional determinations that were performed appear to have adjudicated reporting issues (as we expected given the demonstration procedures), but these reporting-issue determinations had a lower-than-typical probability of yielding a denial. It appears that UI claims examiners may have referred to the demonstration some claimants who missed a required service, without necessarily denying benefits for any period. Conversations with claims examiners also suggest that this was the case, particularly for orientation. Claimants often said that they had not received the notification letter, or that the letter had arrived late. In these cases, the claimants were referred to the next orientation, but benefits were not denied. A number of claims examiners also indicated that denials were issued primarily for repeat offenders rather than for those who missed only one session.

The second measure that provides some evidence about the success of the monitoring and compliance activity is the degree to which claimants who were scheduled for demonstration services but who did not report continued to claim UI benefits. Data that show the extent of this activity are reported in Table VIII.2 for orientation and Table VIII.3 for the job search workshop and assessment/counseling. The first table is broken down by office; the second is not, because the sample size is too small. The data in Table VIII.2 show that 45 percent of the claimants who did not report for orientation stopped claiming UI benefits either before the scheduled session or within one or two weeks.[4] Table VIII.3 shows that the comparable figure for the job-search workshop and assessment/counseling ranges from 32 to 39 percent. No follow-up by UI was expected or necessary for these claimants. The remaining claimants who did not attend a scheduled session did continue to claim benefits. The fact that most of these individuals had a claim date more than 5 weeks after they were expected to report for services suggests that the process of either referring them to services and/or denying them benefits may not always have been successful. However, when we examined these cases focusing on claimants who did not attend orientation, we found that most either had

---

[4] There is evidence that the process of identifying and following up on claimants who did not report for services improved during the life of the demonstration both because the process itself took some time to work out at the local level and because the central office devoted greater attention to monitoring the activity during the demonstration. Data not reported in the tables indicate that the percent of claimants who did not report to orientation but who claimed benefits more than 5 weeks after the scheduled session declined between the first two quarters of sample selection and the last two quarters.

TABLE VIII.2

LAST UI CLAIM DATE RELATIVE TO SCHEDULED ORIENTATION FOR CLAIMANTS NOT ATTENDING ORIENTATION

(Percent)

	Paterson	Hackensack	Jersey City	Butler	Bloomfield	Newark	Elizabeth	Perth Amboy	Burlington	Deptford	Total
Prior to Scheduled Orientation	25.9	41.6	28.4	50.3	45.8	34.0	46.2	33.2	50.3	32.6	37.5
1-2 Weeks Later	7.9	3.7	3.3	13.2	9.6	5.3	8.1	7.1	11.5	10.0	7.6
3-5 Weeks Later	3.2	7.0	5.0	4.7	3.6	3.7	1.4	5.6	3.6	6.2	4.4
More than 5 Weeks Later	63.0	47.7	63.3	31.8	41.0	57.0	44.3	54.1	34.6	51.2	50.5
Sample Size	216	214	181	129	166	321	210	196	165	211	2,009

TABLE VIII.3

LAST UI CLAIM DATE RELATIVE TO SCHEDULED JOB SEARCH WORKSHOP OR ASSESSMENT/COUNSELING INTERVIEW FOR THOSE NOT ATTENDING

(Percent)

	Job Search Workshop	Assessment/Counseling Interview
Prior to Scheduled Event	25.7	22.6
1-2 Weeks Later	13.6	9.7
3-5 Weeks Later	6.8	5.2
More than 5 Weeks Later	53.9	62.5
Sample Size[a]	738	307

[a] The sample in each case consists of claimants who were scheduled for the job search workshop or the assessment/counseling interview but who did not attend.

a nonmonetary determination or had some reason why a determination was not necessary. More specifically, we found that:

- Fifty-one percent of the claimants who did not attend orientation and who continued to claim UI benefits had a non-monetary determination. About half of these determinations led to a benefit denial.

- About 30 percent of the claimants who did not attend orientation and who continued to claim UI benefits had a gap in their claim history of more than 35 days. Such individuals were not referred to demonstration services when they resumed collecting UI.

- Finally, about 11 percent of the individuals who did not attend orientation and continued claiming UI moved from demonstration to nondemonstration offices, including the interstate office.

A final point to note about these data is that some variation exists among the sites. Butler and Burlington showed the lowest percentages of individuals who claimed benefits more than 5 weeks after missing an orientation session, while Paterson and Jersey City showed the highest. These differences are probably due partly to the fact that the local economies were stronger in Butler and Burlington than in Paterson and Jersey City, but they are also probably due to the fact that the working relationship between UI and ES staff was quite good in Butler and Burlington. In both sites, the offices were in the same building, and staff had constant interactions; moreover, the UI claims examiner in Burlington instituted a procedure to call-in claimants as soon as they missed orientation, rather than waiting until they appeared in the office on their reporting date. In this way, these claimants were referred immediately to the next orientation session. In the other two sites, the working relationship was not as smooth initially, but, in fact, the data by quarter show that these two sites improved dramatically over time.

C. ASSESSMENT OF THE MONITORING AND COMPLIANCE PROCESS

This chapter has presented some evidence that the demonstration reporting requirements were enforced. The probability that a claimant's UI eligibility was questioned did increase for treatments relative to controls, and it was due primarily to the failure to report for demonstration services. The probability of benefit denial also increased, although not to the same degree. The process followed by the claims examiners appeared to emphasize referrals to demonstration services when a scheduled service was missed, rather than the automatic denial of UI benefits, particularly for first offenders. There was also evidence that some claimants who did not report for services continued to claim UI benefits, but it appears that most of these individuals either had their eligibility questioned or had an acceptable reason why this was not done. Thus, few individuals appear to have fallen through the "cracks" and avoided the demonstration requirements. We also found evidence that the monitoring and compliance process improved during the demonstration.

Our review of the monitoring procedures and of the experience in the individual sites indicated that the success of the monitoring process required close cooperation between the UI claims examiners and the ES staff. The Delinquency Reports were also quite important, in that they systematically identified claimants who had not complied with the initial reporting requirements. In contrast, the JSA follow-ups were not monitored systematically, and, as discussed in Chapter V, UI was often not notified when claimants did not report for the follow-ups. Nevertheless, the Delinquency Reports were not sufficient in themselves. In many instances, either ES or UI staff had contact with claimants who had not reported, and it became necessary to annotate the Delinquency Reports so that UI and ES staff would know that claimants were expected to attend later sessions or had explicitly been excused from services. It would be possible in future applications to improve the Delinquency Report by focusing only on claimants who had just missed a service in the last week (perhaps with less frequent follow-ups for those who had missed services in the past) and by adding more information including "a remarks section" to the tracking system on the reasons for missed sessions.[5] These steps would make the report shorter and more informative. However, even with these improvements, successful monitoring would still require close cooperation and communication between the UI and ES staff assigned to the monitoring function.

---

[5] A set of codes describing the reasons for missed sessions could be added, but since the reasons were quite varied, providing the ability to include text describing these reasons would also be useful. The text could be printed out with the delinquency report.

IX. ORGANIZATION AND STAFFING

An important element of the demonstration was that it strengthened linkages among the UI system, the ES, and the JTPA programs at both the state and the local levels. UI identified eligible claimants and referred them to demonstration services and monitored their compliance with the demonstration reporting requirements. ES staff provided all the reemployment services, with the exception of training referrals, which were made by staff from the local JTPA Service Delivery Area (SDA) program operators.

In this chapter, we discuss issues associated with the organizational and staffing elements of the demonstration. The first section describes the organizational structure and staffing arrangements of the demonstration. The second section then describes how project staff were trained. The final section discusses staffing issues in general and assesses the degree to which the linkages among the three main programs were achieved.

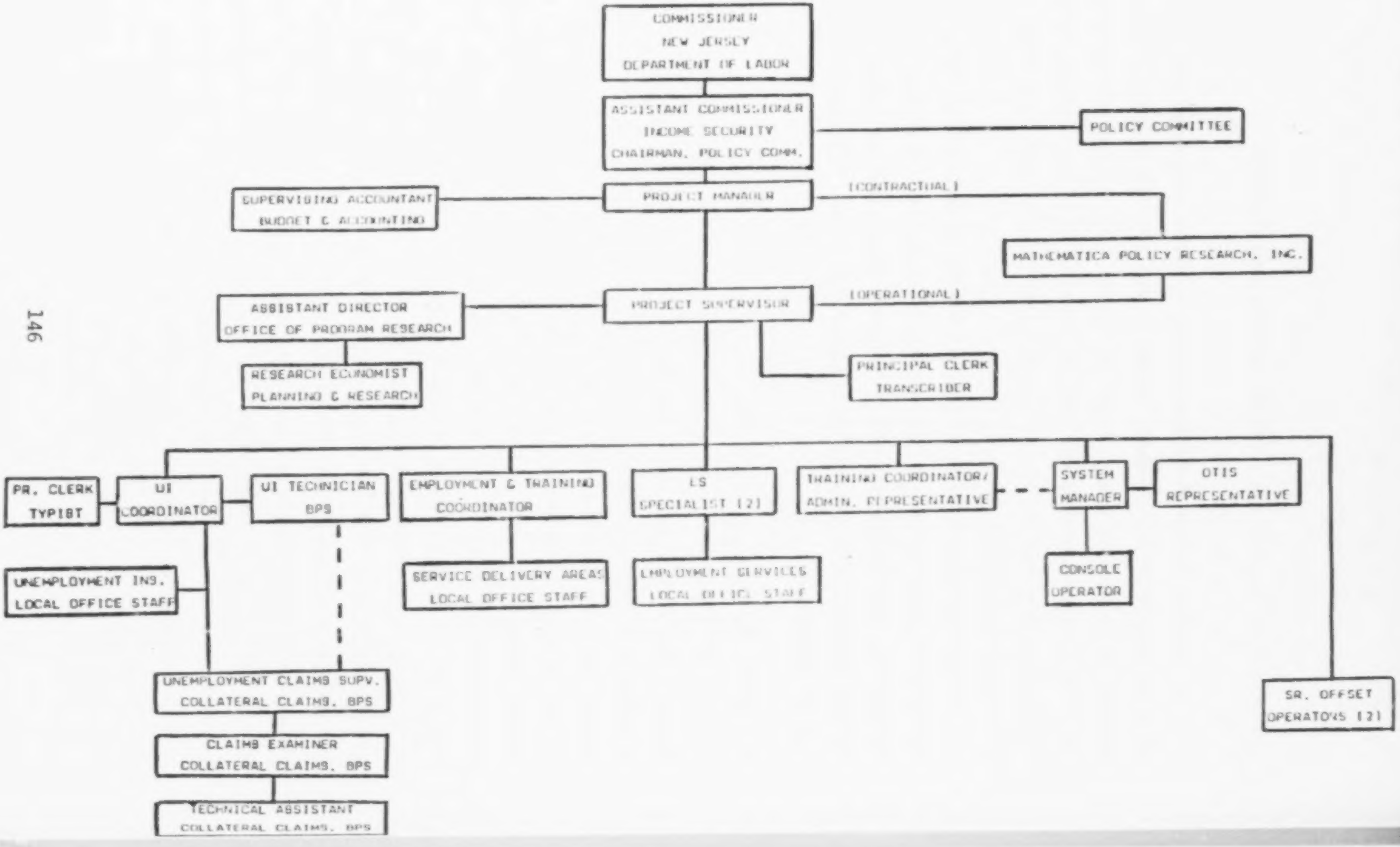
A. ORGANIZATIONAL STRUCTURE

The NJUIRDP required the close cooperation of staff from a number of NJDOL divisions and programs, both centrally and at the local level. The organizational structure developed for the demonstration is shown in Figure IX.1 for the central office staff. As shown in the figure, the demonstration operated under the overall supervision of the NJDOL Commissioner of Labor and the Assistant Commissioner for Income Security who, at that time, had responsibility for UI and ES. The Assistant Commissioner chaired the Policy Committee, which approved the demonstration design and any subsequent changes in policy. This committee included a representative from the USDOL and the heads of each of the major NJDOL divisions that participated in the demonstration.

During the implementation phase, day-to-day operation and supervision of the project was the joint responsibility of the Project Manager, who handled primarily administrative matters, and the Project Supervisor, who was concerned primarily with the actual delivery of services to claimants. The Project Manager was in the Office of Income Security; the Project Supervisor was drawn from the Employment Service. The evaluation contractor, Mathematica Policy Research, Inc., reported to the Project Manager for contractual issues, and to the Project Supervisor for operational issues. During the implementation phase, the evaluation contractor monitored the delivery of demonstration services and

FIGURE IX.1

UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT
NEW JERSEY DEPARTMENT OF LABOR
ORGANIZATION OF ADMINISTRATION
CENTRAL OFFICE
PHASE II - IMPLEMENTATION


COMMISSIONER
NEW JERSEY
DEPARTMENT OF LABOR
ASSISTANT COMMISSIONER
INCOME SECURITY
CHAIRMAN, POLICY COMM.
POLICY COMMITTEE
PROJECT MANAGER
(CONTRACTUAL)
SUPERVISING ACCOUNTANT
BUDGET & ACCOUNTING
MATHEMATICA POLICY RESEARCH, INC.
PROJECT SUPERVISOR
(OPERATIONAL)
ASSISTANT DIRECTOR
OFFICE OF PROGRAM RESEARCH
RESEARCH ECONOMIST
PLANNING & RESEARCH
PRINCIPAL CLERK
TRANSCRIBER
PR. CLERK
TYPIST
UI
COORDINATOR
UI TECHNICIAN
BPS
EMPLOYMENT & TRAINING
COORDINATOR
ES
SPECIALIST (2)
TRAINING COORDINATOR/
ADMIN. REPRESENTATIVE
SYSTEM
MANAGER
OTIS
REPRESENTATIVE
UNEMPLOYMENT INS.
LOCAL OFFICE STAFF
SERVICE DELIVERY AREAS
LOCAL OFFICE STAFF
EMPLOYMENT SERVICES
LOCAL OFFICE STAFF
CONSOLE
OPERATOR
UNEMPLOYMENT CLAIMS SUPV.
COLLATERAL CLAIMS, BPS
CLAIMS EXAMINER
COLLATERAL CLAIMS, BPS
TECHNICAL ASSISTANT
COLLATERAL CLAIMS, BPS
SR. OFFSET
OPERATORS (2)

provided technical assistance when necessary. NJDOL research staff also helped monitor demonstration activities, as shown on the organization chart.

Each of the major actors in the delivery of services reported to the Project Supervisor. These individuals included the UI coordinator, who oversaw and monitored UI operations in the local offices with the assistance of a UI technician; the ES technical assistants, who, with the Project Supervisor, oversaw and monitored ES local office activities; and the Employment and Training Coordinator, who monitored the operations of the JTPA local offices. In addition, the UI Coordinator oversaw the payments process, which was operated by UI Collateral Claims. The System Manager operated the MicroVax and the Participant Tracking System and coordinated the weekly sample extract with the Office of Telecommunications and Information Systems. Finally, the Training Coordinator trained the local office staff in conjunction with the Project Supervisor. This individual also represented the Administration Division and helped monitor operations in the local offices, particularly as they interfaced with the tracking system.

As this description makes clear, the primary actors at the central office level came from a number of different divisions, and it was necessary to establish a good working relationship among the various divisions to ensure the smooth delivery of services at the local level. Achieving this cooperation was made difficult not only by the large number of divisions involved in the project but also by the fact that these divisions reported to three separate Assistant Commissioners[1] and by the fact that there was relatively little prior experience with large, multiple division projects or programs within NJDOL.

Several mechanisms were established in an attempt to overcome these problems and develop good working relationships among the various divisions. First, through the establishment of the Policy Committee, senior managers of NJDOL (the relevant Assistant Commissioners and division directors) were given a forum to express their views and to resolve any major problems. In practice this committee met several times during the design period and periodically throughout the implementation period. In general, the senior managers themselves rather than their representatives took part in the meetings. Second, cooperation was further fostered by developing a Working Group that consisted of the Project Manager, the Project Supervisor, the UI Coordinator, the Employment and Training Coordinator, the Training Coordinator, and the System Manager, as well as representatives from NJDOL Planning and Research, USDOL, and the evaluation contractor. This group met quite frequently, particularly early in the demonstration to address implementation problems. Finally, specific coordination problems at individual sites were addressed at meetings among the relevant central office staff.

---

[1] Most importantly ES and UI reported to one Assistant Commissioner while Employment and Training, which oversaw the JTPA system, reported to another.

The organizational structure at the local office level is shown in Figure IX.2. Two aspects of this chart bear special note. First, the local office UI, ES, and JTPA staff who worked on the project reported to the UI, ES, and JTPA project coordinators at the central office level for project-related activities. However, these central office staff members were not the local office staff supervisors. Instead, in each case, local office supervision was provided as it was under the regular program structure. In the ES and UI systems, the immediate supervisor for the local office demonstration staff was generally the assistant manager, who reported to the local office manager, who in turn reported to the district office. For JTPA, the local operation under a contract from the NJDOL. This program operator supervised the SDA staff assigned to the project.

The second point to be noted about the local offices is that the ES staff and the SDA Assessor were expected to operate as a team in delivering services. The ES counselor, who was assigned to the project full-time, was expected to function as the team leader, and had overall responsibility for providing services. However, this individual did not have direct line supervisory responsibility for any of the other team members. In most offices, the ES counselor and the interviewer, who was also full-time, provided the bulk of the front-end services, although the intermittent interviewers who were half-time often helped with orientation and/or testing.[2] The ES counselors administered all of the counseling/assessment interviews to treatment group 1 and 3 members, and, in many offices, the interviewer was responsible for leading the workshop. The job match specialist was assigned half-time and performed data entry for the project.[3]

An SDA counselor was also assigned to the team about three-quarters time to work with treatment 2 claimants to arrange training referrals. In designing the demonstration, it was expected that the SDA staff member would perform the counseling/assessment interviews for the treatment group 2 members, so that those interested in training would not have to see more than one staff member. However, with the exception of one office, this expectation was not realized, because the SDA staff were not qualified or trained to interpret the GATB results. Thus, treatment group 2 members were generally

---

[2] Because these intermittent interviewers were shared between two demonstration offices, they actually worked full-time on the project.

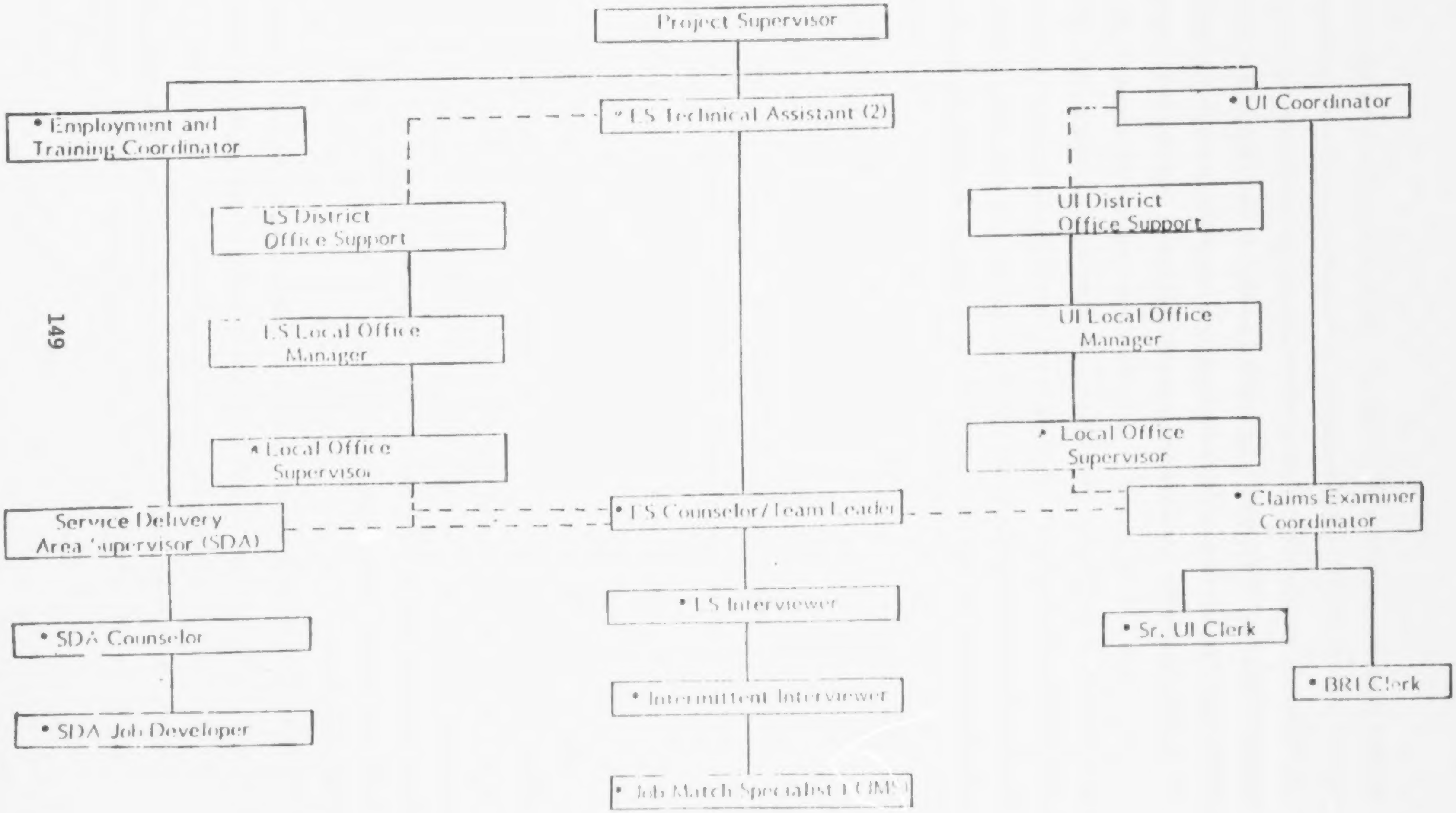
[3] As the project progressed, the counselors and interviewers also performed data entry on the tracking system.

FIGURE IX.2

UNEMPLOYMENT INSURANCE REEMPLOYMENT DEMONSTRATION PROJECT

Organization of Local Office

Phase II - Implementation


Project Supervisor
• Employment and Training Coordinator
• ES Technical Assistant (2)
• UI Coordinator
ES District Office Support
ES Local Office Manager
• Local Office Supervisor
UI District Office Support
UI Local Office Manager
• Local Office Supervisor
Service Delivery Area Supervisor (SDA)
• ES Counselor/Team Leader
• Claims Examiner Coordinator
• SDA Counselor
• SDA Job Developer
• ES Interviewer
• Intermittent Interviewer
• Job Match Specialist I (JMS)
• Sr. UI Clerk
• BRI Clerk

• Titles charged to the Demo Project

seen first by the ES counselor for an introduction to treatment 2 services and an interpretation of the tests, and second by the SDA staff member for a discussion of and referral to classroom training and OJT. In some offices, the interviews were held concurrently, but in others separate visits were required. In several offices, the SDA interview, or a part of it, occurred at the SDA office, rather than at the demonstration office as was envisioned in the design. In most sites, the SDA counselors did not arrange OJT positions directly but instead worked with a job developer in the SDA unit.

A separate staff performed UI local office operations for the demonstration. The claims examiner, who was assigned 40 percent of the time on the project, coordinated this activity and dealt primarily with the claimants who did not report for demonstration services. The UI clerks, each of whom was assigned 20 percent time, handled the collection of New Claimant Questionnaire data at the Benefits Rights Interview, and they assisted the Claims Examiner in pending claims, if necessary.

B. STAFF TRAINING

An important activity in the demonstration was training the staff on (1) the importance of the demonstration, (2) its objectives, (3) its overall design, and (4) the procedures that were to be followed for its successful implementation. The initial training session relied on a procedures manual and training materials, and on-going training sessions were held as the procedures manual was updated.

The procedures manual described first the overall design and then each treatment component. It provided a step-by-step guide to the delivery of services, and it provided copies of the forms required for the project. A separate manual was also prepared for the job search workshop, which provided an agenda for the workshop and a number of exercises that could be used. Since some procedures changed during the demonstration or required more detail, a method was developed to update the manual, based on the Participant Tracking System. When the offices logged on to the system through the terminals located in the local offices, they were informed about the existence of any new procedures. These procedures, which were prepared in a question-and-answer format, were then printed out and added to the manual. This proved to be an effective way to transmit information to the field in a timely manner.

Staff training occurred both initially and on an ongoing basis. The initial training was provided to (1) central office supervisory staff, (2) local office managers and assistant managers, and (3) local UI, ES, and JTPA staff who provided the demonstration services. The two management groups received an overview of the demonstration design to familiarize them with the project and to allow them to provide input to address any perceived problems with operational issues. The local office UI, ES, and JTPA staff[4] then received the overview, together with detailed sessions on the procedures. Several sessions were held for the different types of staff assigned to the project. These sessions were held for the UI claims examiners, the data entry staff, and the ES-JTPA team who provided the reemployment services. Another session was also held for the payments staff. All these sessions lasted about a half-day, with the exception of the sessions for the ES-JTPA team. Since the bulk of the procedures applied to this group, their training session lasted about 3 days.

Training also continued on an ongoing basis for new staff and to address new issues or reinforce old ones. This ongoing training was provided in two primary ways. First, ES, UI, and JTPA central office staff made numerous site visits during the demonstration in which they trained new staff or trained old staff if incorrect procedures were observed. Second, a number of meetings were held with the same level staff from all local offices to introduce any new or modified procedures and to reinforce the consistent application of other procedures. Several of these sessions also brought together staff from ES and JTPA or ES and UI to help foster a good working relationship. In general, these sessions were viewed as quite productive. Finally, as mentioned earlier, old procedures were clarified and new procedures were explained in writing through the computer system used for the project.

C. ORGANIZATIONAL AND STAFFING ISSUES

The NJUIRDP design required that central office staff from a number of separate divisions and local office staff from UI, ES, and the JTPA local program operators work closely together to identify eligible claimants and to deliver services to them. As described above an organizational structure was developed and joint staff training was performed to foster these working relationships. However, one must ask whether the necessary linkages and working relationships did, in fact, develop.

At the central office level the answer to this question is clearly yes. Good working relationships were established among the members of the Working Group, which included the individuals directly responsible for implementing the demonstration. Frequent meetings of this group were held, particularly early in the implementation phase, and there were many smaller meetings and conversations among individuals from the various divisions as operational issues arose. The generally smooth and cordial interactions among Working Group members were probably due to two main factors. First, the individuals themselves were easy to work with and they approached the project enthusiastically and with a spirit of cooperation. Second, the Working Group members could, in many instances, make decisions on behalf of their divisions and when they could not, the division directors were easily accessible to them so that decisions could be made. It is likely that high level departmental officials might be less accessible in an ongoing program which might not generate, on a continuing basis, the kind of interest that was shown by these officials in the demonstration.

---

[4] In many offices, the supervisors of the staff who delivered the services also received the more extensive training.

This latter point applies not only to the top departmental officials but to all staff involved in the demonstration. That is, the morale of both the central and local office staff who were assigned to the project was high throughout the demonstration. Most staff enjoyed working on a special project which was attempting to develop new approaches to delivering UI services. Occasionally, some staff appeared to be uninterested and unmotivated, but they were the exception, and in most cases these staff were reassigned early in the demonstration. The high staff morale was, of course, favorable for the project, but to the degree this staff interest was due to the demonstration nature of the project an ongoing program might encounter less motivated staff and, consequently, might function less well.

Turning more specifically to local office staff, we can ask whether there was a high degree of cooperation between UI and ES for initial data collection and compliance monitoring and between ES and JTPA for service delivery. All the offices developed the formal linkages in which NCQs and delinquency reports were transmitted between UI and ES and in which treatment 2 members were provided services by ES and then JTPA. However, the degree to which staff from the three programs worked together as a team varied, as could be expected, by office. In some sites working relationships between UI and ES staff or ES and JTPA staff were close and there was a high level of communication, while in others there was relatively little interaction beyond the minimum that was needed to transmit information or to refer claimants back and forth between agencies. These differences among offices were probably due, in large part, to differences in the personalities of the various staff members which would vary among sites in any program. However, three more general points about local office staffing and organization can be made.

First, developing working relationships among disparate organizations and individuals takes time and only so much can be accomplished in a limited duration demonstration. Moreover, because there was considerable staff turnover early in the demonstration, the time available to develop working relationships was somewhat less than the full demonstration implementation period in most sites. However, only a few changes were made after the first several months in the key service delivery positions--the counselors, the interviewers, and the JTPA assessors. The major exception was in Paterson, where the entire ES staff changed early in 1987, due to the retirement of the counselor and interviewer. The ES counselor also changed in Deptford. A final set of staff changes which affected working relationships occurred late in the project when the closing of several offices led to substantial shifts at the UI claims examiner position.[5] These shifts affected about half of the offices.

Second, the lack of direct supervisory authority for the local team leader (i.e., the ES counselor) which was mentioned earlier was a problem at times. These individuals could not directly instruct either the ES staff or, of course, the JTPA staff to perform certain tasks. Nor were the specific roles of the ES staff, in particular, completely spelled out in the design. It was expected that each site would allocate the tasks in a way that best utilized the talents of the staff. Most of the time, this was not a constraint, and good working relationships were developed, but at times problems did arise. In those situations, a more structured division of tasks might have helped resolve the problems.

Finally, the fact that the demonstration was operated from the central office but staff were supervised at the local level meant that the organizational arrangements for resolving problems and enforcing authority were not clear. They relied more on the good will of the staff to seek a solution than on formal organizational arrangements. In the case of JTPA there was a further barrier to overcome in that the local staff worked for the local service delivery organization which operated under contract to NJDOL and the central office staff concerned with JTPA worked directly for NJDOL. Nevertheless these central office staff generally worked directly with the local JTPA staff assigned to the project rather than through their line supervisors. Problems related to this division of authority also occurred when ES managers assigned nondemonstration tasks to demonstration staff whom the counselor had expected to be working on the NJUIRDP. For the most part, this situation appeared to be a problem initially, when the workload was not completely built-up, and the managers perhaps felt that these staff were underutilized.

---

[5] Only one demonstration office, Butler, was closed (in June 1987), but closings elsewhere led to the staff shifts.

X. COSTS OF THE INTERVENTION

One important aspect of the demonstration evaluation is to document and assess the costs of providing the three treatments. This information is essential both for determining the degree to which each of these treatments would be cost-effective on an ongoing basis--both from the perspective of the UI system and from the perspective of society as a whole--and for comparing the relative cost-effectiveness of the three treatments.[1] Moreover, cost information is essential for planning and budgeting future programs that are to be implemented on an ongoing basis.

Based on data on demonstration expenditures, this chapter presents quantitative information on the likely magnitude and allocation of resources involved in implementing each of the experimental treatments on an ongoing basis.[2] This information supplements the discussion and analysis of service provision contained in Chapters IV through VII. We begin by discussing the issues involved in estimating the costs of an ongoing program based on demonstration expenditure data. The second section presents benchmark estimates of the treatment payments and operational expenditures per eligible claimant for the three treatment groups. We then discuss the sensitivity of these estimates to alternative assumptions. The final section summarizes the estimates and compares them with the costs of similar programs.

A. ESTIMATING PROGRAM COSTS

The total expenditures for the demonstration were substantial; excluding expenditures for the evaluation, approximately $3.5 million was allocated by the U.S. Department of Labor for operating the demonstration. About one-quarter of the demonstration funds were allocated to training payments, relocation and out-of-area job search assistance, transportation allowances, and the reemployment bonus. JTPA also drew on other sources (primarily Title III) to fund some of the claimant training. The remaining three-quarters of the demonstration funds were allocated to implementing the demonstration and providing the interventions.

---

[1] This benefit-cost analysis is presented in the impact and benefit-cost report. The administrative cost estimates presented here are used in that analysis.

[2] An alternative method of estimating the cost of an ongoing program would be to develop explicitly a budget for such a program. Instead we have used actual expenditures in the demonstration as a guide to the likely magnitude of expenditures in an ongoing program.

While this budget, and the expenditures incurred within it, indicate the overall scale of the demonstration, there are several reasons why demonstration expenditures provide a poor guide to the level of resources that would be required to fund an ongoing program. First, there are a number of demonstration expenditures that would not be incurred in an ongoing program. These demonstration specific expenditures include, for example, the cost of the evaluation. Second, the demonstration expenditures alone do not necessarily encompass all costs or savings which should be ascribed to the demonstration treatments and which would be incurred in an ongoing program. An example here, is the training costs that were paid for out of Title III funds as opposed to demonstration funds. Finally, the efficiency with which the demonstration project was administered might be less than could be achieved in an ongoing program. For example, the same central office supervisory structure used in the demonstration might be sufficient to supervise a program covering more than the ten offices used in the demonstration or more claimants in each office.

In the remainder of this section we address the first issue raised above by indicating which demonstration costs we have chosen to exclude from our benchmark cost estimates for an ongoing program. We do this by identifying divisions or individuals whose functions during the demonstration would likely not be included in an ongoing program. We also address the second issue by including training expenditures from all sources (including the non-demonstration sources) in our cost estimates, but we do not include any other non-demonstration costs in the estimates, since we believe that most operational costs were paid for by the demonstration.[3] The third issue identified above is addressed more explicitly later in the chapter when we provide a number of sensitivity tests.

We begin this discussion of methodology by discussing the payments associated with providing the treatments. The estimation of operational costs is then described in considerable detail, since estimating these costs required that a number of assumptions be made. Finally, we conclude by discussing how the various cost components were added together to produce per-claimant cost estimates by treatment group.

1. Treatment Payments

Payments were made to vendors and to claimants for the following treatment components:

o Classroom training

---

[3] The cost estimates obtained under these assumptions are estimates of the gross costs of providing the demonstration treatments. In the Impact and Benefit-Cost report, we present estimates of net costs that take account the fact that some services would have been provided to treatment group members in the absence of the demonstration.

- On-the-job training
- Transportation allowances for classroom training
- Relocation assistance and out-of-area job search assistance
- Reemployment bonuses

Estimating the treatment payments for each treatment package is straightforward, since all these treatment payments would be part of an ongoing program. Moreover, because recording and monitoring these expenditures were part of the ongoing operation of the demonstration, the data are readily available. Classroom and on-the-job training costs were recorded by each JTPA Service Delivery Area (SDA) operator as part of the regular financial monitoring process.[4] Reports on these costs were submitted to the central office JTPA staff. The remaining treatment payments were recorded through the special MicroVax payments system set up for the demonstration (see discussion in Chapter VII).[5]

2. Estimating Operational Costs

Operational costs include the labor and nonlabor costs associated with identifying eligible claimants, offering them reemployment services, and providing them with these services. Although expenditures for these activities were recorded by the New Jersey Department of Labor, using the operational costs of the demonstration to estimate the costs of an ongoing program is difficult, as discussed above, because demonstration-specific costs must be separated from the costs that would be incurred in an ongoing program. For this reason, a number of decisions and assumptions had to be made to generate operational-cost estimates. Our discussion of these issues consists of four sections: (1) start-up versus ongoing operational costs, (2) central office labor costs, (3) local office labor costs, and (4) other operational costs.

Start-Up Versus Ongoing Operational Costs. As with any demonstration, the NJUIRDP encompassed a planning phase and an operational phase. The planning phase, which covered the period from October 1985 to April 1986, focused on the design of and early planning for the demonstration. The operational phase, extending from May 1986 through September 1987, included an initial two-month period in which program materials were developed and staff were trained, 12 months during which claimants were selected and services provided, and a final 3-month period during which the provision of services was phased out.

---

[4] Included here are all training costs regardless of whether they were paid for by demonstration funds or Title III funds.

[5] Some claimants who received training through the JTPA system were not included in these reports. We used average costs of classroom and on-the-job training to estimate treatment costs for these individuals.

Given that we are interested in estimating the costs of an ongoing program, our analysis of costs focuses on the expenditures incurred during this operational phase. Moreover, since we are interested primarily in the costs of an ongoing program after the start-up phase, we have chosen a specific time period within the operational phase on which we base our estimates. This time period is from October 1986 through June 1987. We chose this period because it was the period during which the program was at a steady state; that is, new claimants were selected for the demonstration throughout this period, while a full complement of claimants who had previously been selected were receiving services. The three months of operations prior to this period represented a start-up period both because the caseload was being built up and because the services were just being implemented. The three months after this period constituted the phase-down period during which no new claimants were selected.

However, we also provide an estimate of operational start-up costs--that is, the costs of developing procedures, providing initial training to staff, and organizing the sites. These costs, with some exceptions, are the expenditures incurred during the first two months of the operational phase (May and June 1986). This analysis provides a rough estimate of these costs.

Central Office Labor Costs. The central office staff played several roles during the operational phase--including participating in the demonstration design and decision-making, supervising the demonstration, selecting eligible claimants, facilitating the collection of demonstration data, processing the treatment payments, and conducting independent research of the demonstration. While all these functions were useful in the demonstration, certain functions (such as the research function) would probably not be part of an ongoing program.[8] We excluded such functions from our estimates by examining the functions performed by each division (and in some cases by each individual within a division) and deciding whether the functions performed by these divisions or individuals would be included in an ongoing program. For our initial estimates we either included all the costs or excluded all the costs associated with each individual (or division). In general, we included the costs of the operational divisions (UI, ES, and JTPA) and excluded the costs of other divisions (Planning and

---

[8] Some research functions might be included in an ongoing program, but the scale of the research function in the demonstration clearly exceeded the level that would occur in an ongoing program.

Research, Administration,[7] Finance and Accounting, and the Office of the Assistant Commissioner for Income Security).

Although we believe that this adjustment to central-office costs provides a better estimate of expenditures in an ongoing program than would one that included all costs, there are two reasons that it still may not provide an accurate picture of ongoing expenditures. First, as mentioned above, this adjustment does not take into account the fact that some staff members who were included in the cost calculations may have devoted more time to the demonstration than would have been the case in an ongoing program. Second, the organizational structure of the demonstration required substantial coordination among ES, UI, and JTPA staff at the central office level. Such coordination was expensive in terms of staff time, and the costs might be lower in an ongoing program in which interagency linkages would be established and institutionalized early on, and extensive ongoing program coordination at the central office might not be required. The degree to which these factors affect costs is difficult to determine with any certainty. While our initial estimates of costs ignore these factors, we address them when we discuss the sensitivity of the estimates to alternative assumptions.

In summary, our benchmark estimate of central office costs, based on data provided by NJDOL, includes all expenditures incurred during the October 1986 through June 1987 period by a subset of central office UI, ES, and JTPA staff in supervising the demonstration, as well as central office expenditures incurred in processing payments (for the reemployment bonus, relocation expenses, etc.).

Local Office Labor Costs. Our estimates of local office labor costs for ES and UI staff were also based on expenditure records obtained from the NJDOL. Here, again, we want to exclude expenditures for demonstration features that would not exist in an ongoing program. In particular, the questions on the New Claimant Questionnaire (NCQ), which were collected on a separate form by UI staff and data entered by ES staff, would be included in a standard eligibility form in an ongoing program and would not require special processing. Consequently, our estimates of the costs of processing the NCQ by both UI and ES staff have been excluded from our initial benchmark estimate.[8]

---

[7] Because several members of the Administration Division played operational roles in the demonstration, some costs for this division were in fact included.

[8] Although incorporating the NCQ into the existing system would reduce costs, some costs would still be associated with collecting and data-entering these data items. However, because these costs would likely be negligible, we have not included an estimate of them. There would also be a one time cost associated with including the NCQ variables in the existing database.

We have included all other local UI and ES office costs in our estimates. We also included the costs billed by the JTPA local program operators in our estimate of local office costs.

Other Operational Costs. In addition to labor costs, operational costs include indirect costs such as fringe benefits, general administration, and rent. These costs were estimated on the basis of the rates at which these items were assessed during the period from October 1986 through June 1987.

Operational costs also include direct costs other than labor such as travel to local offices. We have estimated these other direct costs for the time period between October 1, 1986 and June 30, 1987. These other direct-cost estimates were based on actual costs, where data were available, and on budgeted costs, where necessary. These data were adjusted in cases where actual costs did not reflect the costs of an ongoing program. For example, during the demonstration, staff traveled to local offices, conferences, and planning meetings more frequently than would be expected in an ongoing program; therefore, travel costs were estimated by making assumptions about the number of persons who traveled and the frequency of trips necessary to monitor the local offices.

Estimating data processing costs was also not straightforward. Rather than disturb the existing data processing systems used by UI and ES, the demonstration had its own data processing system. Demonstration operations were supported by the Participant Tracking System (PTS), which operated on a MicroVax installed in the central office with terminals in the local offices. This system supported the delivery of demonstration services, provided a convenient data base for monitoring and summary reporting, and provided data to the research data base to be used to evaluate the demonstration.

For an ongoing program, however, data processing would most likely be integrated into the state's existing system. Therefore, our initial benchmark estimate of program costs excludes the costs incurred in downloading data from the New Jersey mainframe to the MicroVax and in operating the MicroVax, as well as all the costs associated with the MicroVax equipment and supplies. Instead, we assumed that the project would occupy half of the available time on one terminal (linked to the NJ mainframe) at each local office, and would thus incur one-half of the monthly per-terminal charge at each local office.[9]

Once again, some uncertainty surrounds this estimate, given that we do not know how realistic these assumptions are. However, the difference between this estimate and the actual costs of the MicroVax system was actually quite small (about $3 per claimant), suggesting that this assumption does not significantly affect our estimates.

---

[9] This estimate was provided by NJDOL staff.

3. Per-Claimant Expenditures by Treatment Intervention

These estimates of treatment payments and operational costs provide total expenditure estimates for these two types of costs for various time periods. To be useful, these estimates must be allocated to the three treatments and placed on a per-claimant basis so that they can be added together. We did so as follows.

The treatment payments were allocated to the relevant treatment packages. That is, payments for classroom and on-the-job training, transportation allowances, and relocation and out-of-area job search were allocated to treatment 2, since these benefits were available only to treatment 2 claimants. Similarly, reemployment bonus payments were allocated to treatment 3. These cost components were summed and then divided by the total number of claimants assigned to treatments 2 and 3, respectively, since the payments data applied to the entire demonstration period. This procedure produced per-claimant treatment payment estimates, where the cost is the payment per claimant assigned to the treatment (it should be noted that no "treatment" costs were incurred for treatment 1).

A similar procedure was followed for operational costs. Since these costs applied to the October 1986 to June 1987 period (with one exception, noted below), we allocated them to the three treatments based on the allocation of the sample over this period.[10] We also estimated the per-claimant cost by dividing the total operational costs for this period by the number of claimants selected over this time period.[11]

Two exceptions were made to this procedure. First, the expenditures for payments (i.e., for the reemployment bonus, etc.) were allocated to the treatments based on the proportion of checks paid to each treatment group (treatment group 1, for example, incurred no payments-processing expenditures.) Second, since the JTPA local office expenditure data applied to the period from July 1986 through September 1987, we divided these expenditures by all treatment 2 claimants to obtain a per-claimant figure.[12]

---

[10] This procedure may overstate the costs for treatment 2, since JTPA counselor staff time, all of which is allocated to treatment 2, substituted for some ES staff time. However, we have not made any adjustment for such substitution, because treatment 2 claimants had contact with ES staff throughout much of the service sequence, including assessment/counseling.

[11] The costs incurred between October and June to provide services to claimants selected prior to October offset the costs incurred after the period to provide services to claimants selected during the period.

[12] The JTPA budgetary data actually included funds for the two months prior to July 1986. We subtracted these funds from the estimate of ongoing costs.

In summary, this process enabled us to estimate total per-claimant program expenditures for each treatment intervention by summing the per-claimant treatment payments and operational costs by treatment package. With this cost estimate, cost comparisons can be made among interventions, and policymakers can more readily predict program costs regardless of the scale of the program. In addition, these cost estimates can be compared with the impact estimates which indicate the average effect of each intervention per claimant. This analysis is presented in the Impact and Benefit-Cost report.

B. TREATMENT PAYMENTS AND OPERATIONAL COSTS PER CLAIMANT

Table X.1 presents our benchmark estimates of the average per-claimant cost of each treatment intervention as implemented on an ongoing basis. As designed, the three interventions represent quite different magnitudes of cost, with per-claimant expenditures of $169 for treatment 1, $491 for treatment 2, and $300 for treatment 3.

As indicated in Table X.1, a major reason for these differences in average costs is that average treatment payments differed for the three treatments. By design, no special services were provided in treatment 1; hence, no payments were incurred for this treatment. For treatment 2 claimants, the special services cost an average of $250 per claimant assigned to the treatment. Most of this cost was devoted to classroom training or on-the-job training, and only small amounts were devoted to transportation allowances, relocation payments, and out-of-area job search assistance. This difference was due to the fact that training is more expensive than these other services, and that a higher proportion of claimants received training (particularly classroom training) than received these other services (see Chapter VI discussion). Claimants in treatment 3 could also receive special payments for the reemployment bonus. This bonus averaged $125 over all claimants assigned to this treatment.[13]

With respect to operational costs, we should note that local office labor costs (including fringe benefits) represent the majority of these costs (70 percent for treatment 1), which is to be expected given that the local offices provided the services to claimants. We should also note that the average per-claimant operational costs for both treatments 2 and 3 exceeded the average operational costs for treatment 1 since these treatments added services to the treatment 1 package. At the local office

---

[13] The maximum bonus that was offered averaged $1,600. The actual average bonus payment per claimant was lower because (1) not all claimants in treatment 3 attended assessment and received a bonus offer, (2) not all claimants who were offered the bonus collected it, and (3) not all claimants who received the bonus received the maximum.

TABLE X.1

ESTIMATED PER CLAIMANT EXPENDITURES FOR AN ONGOING PROGRAM
(In Dollars)

	Treatment 1 Expenditures	Treatment 2 Expenditures	Treatment 3 Expenditures
TOTAL PER CLAIMANT EXPENDITURES	$169	$491	$300
EXPENDITURES, BY TYPE OF COST			
Average Treatment Payments			
Treatment 2			
Classroom training	n.a.	224	n.a.
On-the-job training	n.a.	23	n.a.
Relocation assistance	n.a.	1	n.a.
Out-of-area job search	n.a.	1	n.a.
Transportation allowance	n.a.	1	n.a.
Total	n.a.	250	n.a.
Treatment 3			
Bonus payments	n.a.	n.a.	125
Total	n.a.	n.a.	125
Average Operational Costs			
Local office labor costs[a]	118	183	118
Central office labor costs[a]	25	29	30
Nonlabor costs plus administrative overhead[b]	26	29	26
Total	169	240	174

NOTE: Totals may not add due to rounding.

[a] Labor expenditures include all fringe benefits.

[b] This category includes direct items other than labor, as well as indirect costs.

n.a. = not applicable

level, JTPA administrative costs accounted for the substantially higher costs estimated for treatment 2. The differences at the central office level are due to the fact that the payments-processing costs for the reemployment bonus and other treatment payments were attributable only to these treatments. Central office JTPA staff costs were also attributable only to treatment 2.

In addition to these per-claimant treatment costs for ongoing operations, we also estimated start-up expenditures as a guide for future program planners. We did not place these on a per-claimant basis, since we felt that a total dollar figure would be more useful. We estimated that a program of the scale of the NJUIRDP would incur about $140,000 in operational start-up expenditures. This estimate reflects expenditures for developing program procedures, providing initial training to staff at 10 local offices, and organizing the sites in preparation for implementing the program.[14] Also included in this estimate are the expenditures necessary to incorporate the program's data processing requirements into the state's existing data system.[15]

C. THE SENSITIVITY OF THE ESTIMATES TO ALTERNATIVE ASSUMPTIONS

The initial benchmark estimates presented above provide, we believe, upper bound estimates of the costs of providing the three treatments because these estimates do not take account of the fact that the demonstration may have operated less efficiently than would be the case in an ongoing program. Some efficiency gains might be achieved in central or local office staffing in an ongoing program. To examine the cost implications of these potential gains in operational efficiency we provide, in this section, some alternative cost estimates that are based on fairly arbitrary assumptions concerning the degree to which efficiency gains might be realized. These alternative estimates provide what should be viewed as a set of lower bound cost estimates. It is important to note, however, that while costs might be reduced in an ongoing program relative to the actual expenditures experienced in the NJUIRDP, a reduction in expenditures and in staff effort might also affect program outcomes. Thus these lower bound estimates should be viewed with caution.

The first two alternative estimates or sensitivity tests focused on reductions in central office supervision, since a major area of uncertainty in our benchmark estimates was the degree of central office supervision that one might find in an ongoing program. We speculated that some demonstration staff members might not play as extensive a role in an ongoing program as they played in the demonstration. Moreover, the demonstration may have required more short-term coordination among ES, UI, and JTPA than one might find in an ongoing program. Rather than attempting to develop detailed alternative assumptions using central office staffing, we conducted two simple tests to reduce the size of the central office:

---

[14] As noted previously this estimate is based on actual expenditures incurred for these tasks during the demonstration.

[15] The cost of the data processing charges was estimated by NJDOL staff.

- A 25 percent reduction in central office supervision costs
- A 50 percent reduction in central office supervision costs

These reductions, particularly the 50 percent reduction, appear sizeable, but it is possible that a larger reduction would occur in an ongoing program, since many special programs are run with substantially less central office supervision than implied by either of these assumptions.[16] However, the high level at which central office staff were involved in the NJUIRDP was, we believe, important in ensuring that the procedures were followed correctly, and we are reluctant to assume that this would occur if the level of supervision were reduced to the level that is often used. The 50 percent reduction represents a lower bound.

A third test focused on the scale of the demonstration. Although some of the sites included in the demonstration were relatively small and did not have full claimant loads, they did have the same size staff as the larger offices. In order to determine the manner in which these scale differences affected costs, we estimated per-claimant local office costs based only on the 6 sites with the largest number of treatment-group members (Elizabeth, Newark, Perth Amboy, Hackensack, Bloomfield, and Jersey City).

A final test also focused on the scale of the demonstration, in particular the JTPA operational costs associated with treatment 2. The reason for this focus was that, although approximately the expected number of claimants were assessed in treatment 2, considerably fewer claimants expressed interest in training or entered training than was anticipated when the budget for this activity was developed. Moreover some of the functions that JTPA staff were expected to perform (e.g., test interpretation) were performed by ES staff (see discussion above concerning the reasons for this change from the design). For these reasons more JTPA staff time may have been spent on the project than would be needed in an ongoing project where the caseload might be better anticipated. Since the number of trainees was about 40 percent of the expected number, one might make the assumption that JTPA stafftime could be reduced by 60 percent of its actual level. However, since it is unlikely that the training placement process is subject to constant returns to scale[17], a somewhat smaller reduction in costs is more appropriate. In our estimates we have used a 50 percent reduction in these costs as a lower bound measure.

---

[16] For example, the ES central office supervisory staff for the demonstration consisted of a full-time supervisor, 2 full-time assistants, and a half-time secretary. Ongoing special programs which are spread over all offices (the number of which is about four times as many offices as were included in the demonstration) are often handled in New Jersey by a half-time supervisor and 2 full-time assistants.

Table X.2 presents the results of these sensitivity tests and provides a comparison of these alternative estimates with the initial benchmark estimates. In general, the alternative estimates generated from these tests are similar to the overall magnitude of the benchmark estimates. The 50 percent reduction in central office supervision costs, for example, generated per-claimant estimates that were, at most, 7.7 percent less than the benchmark estimates. This is due to the fact that central office supervision costs accounted for less than 15 percent of total per-claimant expenditures for any treatment.

The results of the third sensitivity test indicate that increased scale within a local office may indeed help reduce costs, at least up to a point. While the average per-claimant local office labor cost for treatment 1 was $132 across the 4 sites with the smallest sample sizes (Burlington, Butler, Deptford, and Paterson), the local office labor cost at the larger sites was $112, on average. Here, again, however, the reductions in total per-claimant expenditures were relatively small.

The results of the reduction in local office JTPA costs were to reduce treatment 2 per-claimant expenditures by $32. As with the other tests this change had only a small impact on total expenditures.

The final line in Table X.2 shows the effect of assuming that both 50 percent reductions could be achieved and that the sites would operate on a larger scale. The combined effect of these changes obviously yields a larger reduction in costs than any single adjustment. These adjustments yield per-claimant cost estimates of $150 for treatment 1, $438 for treatment 2, and $280 for treatment 3. These estimates represent reductions from the initial benchmark estimates of 7 to 11 percent depending on the treatment. While these reductions sound modest and further cost savings might be achieved in an ongoing program, we believe that it is useful to view these estimates as providing lower bound estimates of per-claimant costs and to view the initial benchmark estimates as providing higher bound estimates.

---

[17] That is, the cost of placing, for example, 100 individuals is less than two times the cost of placing 50 individuals.

TABLE X.2

ALTERNATIVE ESTIMATES OF PER CLAIMANT EXPENDITURES (in dollars)

	Treatment 1 Expenditures		Treatment 2 Expenditures		Treatment 3 Expenditures	
Initial Benchmark	$169		$491		$300	
25% Reduction in Central Office Supervision	163	(-3.6)	484	(-1.4)	294	(-2.0)
50% Reduction in Central Office Supervision	156	(-7.7)	476	(-3.1)	285	(-5.0)
Larger Scale of Offices	163	(-3.6)	478	(-2.6)	295	(-1.7)
50% Reduction in Local Office JTPA Costs	169	(0.0)	459	(-6.5)	300	(0.0)
50% Reduction in Central Office Supervision and in Local Office JTPA Costs and Larger Scale	150	(-11.2)	438	(-10.8)	280	(-6.7)

NOTE: Numbers in parentheses represent the percentage change in the benchmark estimate.

D. SUMMARY AND CONCLUSIONS

Our analysis of demonstration expenditure data indicated that the three interventions represented quite different magnitudes of cost. Our initial benchmark estimates indicated that the basic set of services would cost $169 per claimant in an ongoing program, bonus payments would add about $125 to the basic cost estimate for treatment 3 (with total per-claimant expenditures of about $300). Treatment 2 was the most expensive, with total per-claimant expenditures of about $491 (estimated for an ongoing program), both because of the cost of training and because of the increased staff time provided by the local area JTPA program operators. Alternative estimating assumptions led to relatively small reductions in overall costs that ranged from 7 to 11 percent. In addition, demonstration expenditures indicated that a new project of this scale would incur about $140,000 in initial start-up costs.

In the impact and benefit-cost report these per-claimant expenditure estimates are adjusted to take account of the fact that some expenditures for services would occur in the absence of the demonstration treatments. The resulting estimates of net administrative costs are compared with the treatment impact estimates to determine whether the interventions were cost-effective from the viewpoint of the Labor Department (federal and state) and beneficial from the societal and claimant perspectives. The findings are that all treatments were beneficial from the societal and claimant perspectives while none were beneficial from the perspective of the Labor Department. That is, the net costs of providing services exceeded the reductions in UI expenditures.

Another comparison that can be made is to compare the NJUIRDP per-claimant cost estimates with data on the costs of other interventions targeted toward displaced workers. As shown in Table X.3, we can do so for the six dislocated worker demonstrations examined by Corson et al. (1984), the Downriver demonstration examined by Kulik et al. (1984), and the Texas Worker Adjustment Demonstration examined by Bloom and Kulik (1986). Cost data from these evaluations are generally reported on a per-participant basis, as opposed to the per-person offered services basis used for the NJUIRDP. However, if we assume that the 56 percent of NJUIRDP eligibles who attended the assessment are "participants," we can calculate that the average per-participant cost of job search assistance in the NJUIRDP was $302 ($169/.56). This average cost is lower in all but one instance than the average costs reported in these other demonstrations, and that one intervention (the Milwaukee demonstration) provided only job matching services. In most cases, the average cost of the NJUIRDP was substantially less than the cost of these other interventions.[18]

---

[18] We have not compared average costs for the training treatment, since average costs vary substantially with the training participation rate.

TABLE X.3

AVERAGE COST PER PARTICIPANT FOR JOB SEARCH ASSISTANCE FOR SELECTED DISLOCATED WORKER DEMONSTRATIONS
(dollars)

Demonstration	Average Cost
Six-Site Demonstration	
Alameda	n.a.
Buffalo	$ 851
Lehigh Valley	407
Mid-Willamette Valley	1,133
Milwaukee	73
Yakima	1,387
Downriver	624
Texas	
Houston	1,530
El Paso	370

n.a. = not available.

SOURCE: Data for the six-site demonstration are reported in W. Corson et al., "Process and Implementation Issues in the Design and Conduct of Programs to Aid the Reemployment of Dislocated Workers," Mathematica Policy Research, Princeton, N.J., October 30, 1984, p. 91. Data for the Downriver and Texas demonstrations are reported in Howard S. Bloom and Jane Kulik, "Facilitating Adjustment to Worker Dislocation: The Record to Date," paper prepared for the 1987 Conference of the Association for Public Policy Analysis and Management, Bethesda, MD, October 1987, Table 3.

XI. REPLICABILITY AND SUMMARY

Based on the process and implementation analysis presented in the previous chapters, the demonstration treatments were generally implemented as designed. Eligible claimants were selected and randomly assigned to the three treatment groups and the control group, and these eligible claimants were offered the services that were specified in the design. They were notified to report for orientation, and, when they did, they generally progressed through the initial sequence of services--orientation, testing, the job search workshop, and the assessment/counseling interview. These services were provided early in the claims spell, thus achieving the goal of early intervention. When claimants did not report for orientation or the other services, UI staff were informed and the claims files were pended, and claimants were referred for demonstration services when they continued to file UI claims. Individuals who attended the assessment/counseling interview were offered additional services depending on their treatment group, and many of them did receive these additional services--the job search follow-ups, training, or the reemployment bonus.

This favorable assessment must be tempered somewhat. Not all claimants reported for the services, and not all of this nonreporting group was identified and followed up in a timely fashion. A number of claimants were also excused from some or all of the services. In addition, not all claimants were followed as scheduled after they completed the assessment/counseling component, and training participation rates were not as high as anticipated in most sites. Moreover, the ten sites varied somewhat in how they provided the services. This variation was observed in the following areas:

- Sample selection was not performed as timely as desired in several of the large offices, due to lags in the entry of New Claimant Questionnaire data. Consequently, the services were not provided as early in the claim spell in these offices as they were in the others.
- Policy on the excusal of claimants from one or all services seemed to vary substantially by office, making the degree to which the selected claimants received services vary by office. Although this variation was substantial in some instances, the types of claimants who were excused, particularly those who expected to be recalled, would likely be largely eliminated from an ongoing program, and the observed variation would not be as great.
- The extent to which the UI-ES monitoring and compliance process identified claimants who did not report for services and successfully referred them back for services also differed by site, but all sites did seem to perform this function, as measured by the increase in the eligibility determination rate.
- The degree and nature of the JSA follow-ups also varied, primarily in the use of the resource center. The centers appeared to be used in four sites, but not to any great extent in the others.

- The training participation rate also varied substantially by site; three sites were more successful than the others at placing claimants in training. Some variation was expected, however, and any concern about training pertains more to the overall level of participation rather than to the differences among sites.

- Finally, the reemployment bonus receipt rate was relatively uniform by site, with one exception, where it appears that a somewhat more extensive job verification process was adopted which led to a lower receipt rate than in the other sites.

However, these site differences do not appear to be substantial enough to be of major concern for the analysis. The observations of no single office differed from the others to the extent that its sample should be dropped from the analysis. In addition, the variation that was observed is likely to occur in an ongoing program; indeed, the variation is likely to be greater.

Our review of the NJUIRDP also leads to several observations about the replicability of the demonstration treatments in ongoing programs:

- During the demonstration period, the economy in New Jersey was strong and dynamic, with a low unemployment rate of 5 percent. Although employment declined in some sectors (namely, manufacturing) and in some sites (Paterson, Newark, Jersey City, and Perth Amboy had relatively high unemployment), the overall employment picture was strong, with substantial growth occurring in the service and trade sectors. This situation must be kept in mind when the demonstration results are interpreted.

- The demonstration was conducted in 10 offices which were large enough to provide a steady flow of claimants to the program. If an ongoing program were conducted in all offices, operations in smaller offices would either be less efficient or services would need to be delivered on a less frequent basis (e.g., bi-weekly instead of weekly). A less efficient operation would increase costs and less frequent provision of services might reduce impacts because it would lengthen the time from the UI claim to when services are provided.

- The selection criteria used in the demonstration encompassed individuals who expected to be recalled but who did not have a definite recall date. These individuals presented some service problems when it became clear that they were truly "job attached." Many were excused from services. If either this group is completely dropped from a future program or better selection criteria can be identified to screen out those whose recall expectations are realistic, some of the operational issues that arose in the NJUIRDP would be avoided.

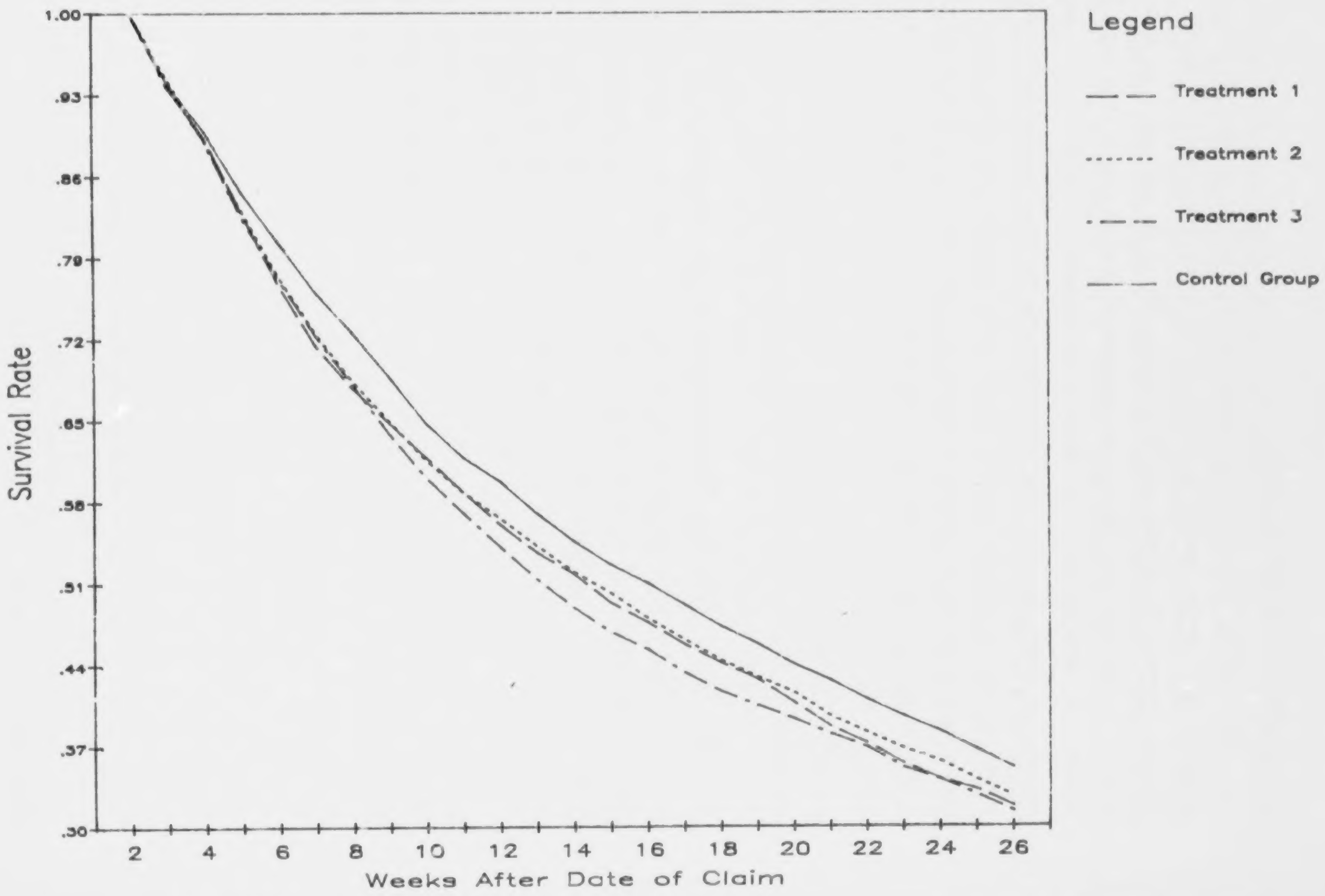
- Because of the demonstration nature of the project, the selection process required that a special data collection instrument be administered to UI claimants, and that the data that were collected be entered in the demonstration's computer system. Sample selection was a two-part process, based on both the regular UI data system and the demonstration data system. In an ongoing program, these systems would be combined. If data that were unavailable in the UI system were needed for eligibility determination, it is likely that the data items would be added to the system for the program.

- The Participant Tracking System used in the demonstration was, we believe, a key feature of the program, since it was used to notify local staff about claimants to whom they should be providing services and claimants who did not report for services. This system also provided a monitoring tool for program supervisors. It made it difficult for claimants to avoid contact with the demonstration or for staff to avoid dealing with claimants with whom it was difficult to work. Developing a similar system or set of procedures would be necessary to replicate the demonstration treatments in an ongoing program environment.

- The demonstration instituted a high degree of central office supervision, which was important to ensuring that the services were delivered and that the necessary linkages among the UI, ES, and JTPA systems were maintained. An ongoing program might not have as large a supervisory staff, but we believe that substantial reductions might not yield the same level of service delivery or interagency coordination.

- Finally, the demonstration was highly visible both at the state and the national levels. Numerous site visits were made by USDOL and the evaluation contractor's staff, as well as by senior state staff from New Jersey and, in some cases, other states. These visits and the fact that the importance of the demonstration was promoted to local office staff contributed to the high level of staff enthusiasm cited earlier. We cannot assess the effect of this situation on the demonstration results or on the generally successful implementation, but it may have had a positive impact that would be difficult to replicate in an ongoing program.

FIGURE V.1
NUMBER OF WEEKS IN FIRST UI SPELL - SURVIVAL RATES



An examination of Figure V.1 reveals some interesting differences among the survival rates over time. For the first eight weeks after the claim filing date, the survival rates for the treatment groups were nearly equal. Since the treatments were identical during the first 8 weeks (on average, assessment/counseling interview was held during the eighth week after the claim filing date), this result is expected. However, the plots for this initial time period also show that the difference between the treatment and control groups began to show up at about week five, which was the point at which orientation occurred, and the difference grew rapidly over weeks 5 to 8. This pattern suggests that the treatments (JSA at this point) had an immediate impact on the claimants who were selected for the demonstration treatments.

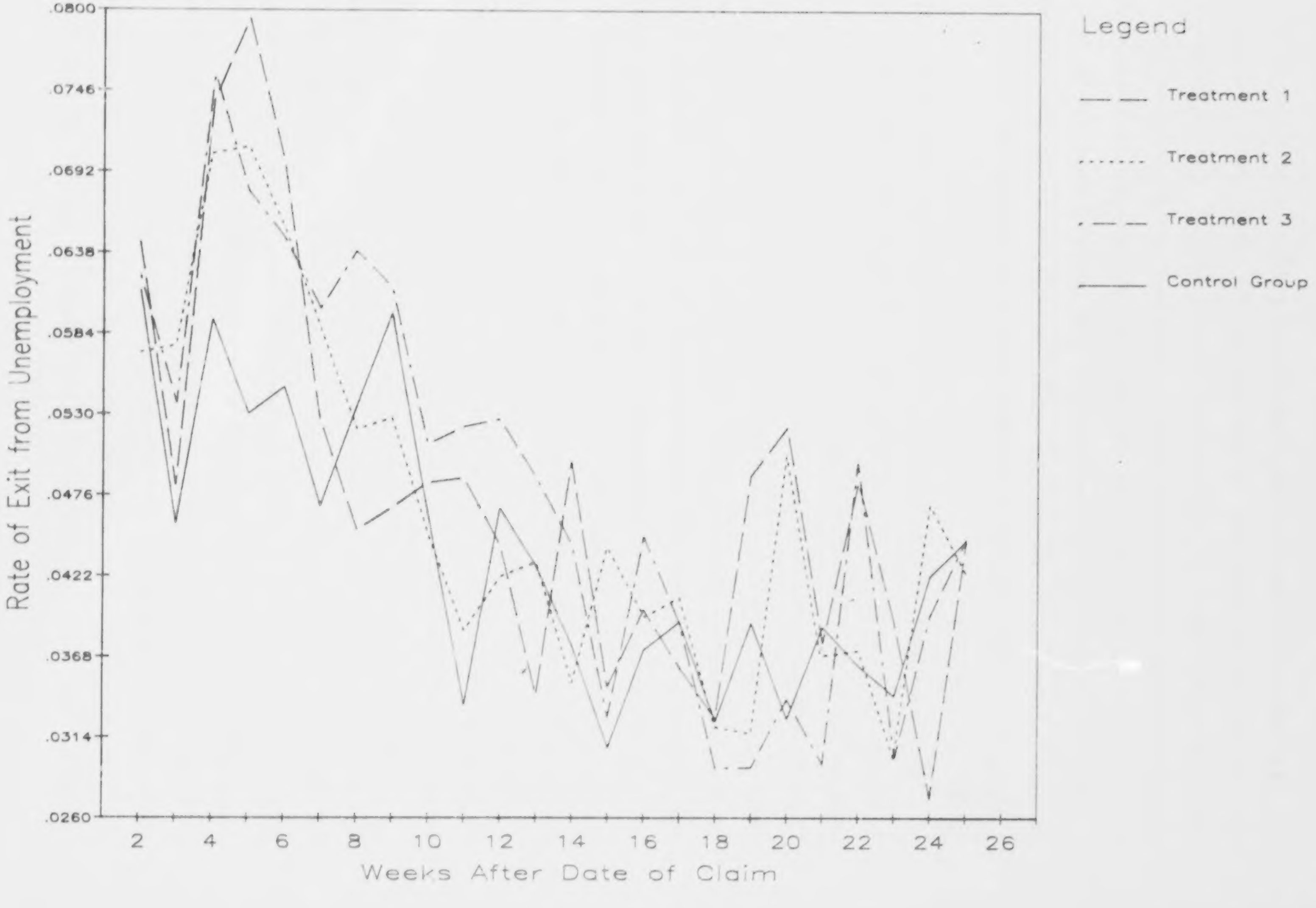
Beginning at about week 9, the survival rate for treatment 3 was noticeably less than the rates for the other treatment groups. This difference reflects the impact of the reemployment bonus offer, which provided an additional incentive to become reemployed quickly. The survival rates for treatments 1 and 3 converged around week 24. This convergence of the survival rates suggests that the reemployment bonus probably had a minor impact on claimants who would have exhausted benefits in the absence of the offer. Instead, the impact appears to have been to shorten the spells of claimants who would not have exhausted their benefit entitlements. For treatment 2, the survival rate plot was almost identical to that of treatment 1, except for the tail, where it appears that slightly more individuals in this treatment exhausted benefits. This result is probably due to the fact that individuals who entered training continued to collect benefits during their training period. A final point to note is that the survival rates of all the treatment groups remained below those of the control group at week 26, suggesting that the treatments had an impact on the duration of unemployment beyond the 26th week following the claim filing date.

Figure V.2 presents the plotted exit rates for each of the treatment and control groups. Because exit rates are volatile over time, it is difficult to draw treatment-control comparisons, but the exit rates do clearly show the effect of the demonstration treatments during the first 8 weeks after the claim filing date. That is, the exit rates of the treatment group exceeded those of the control group. The overall pattern of the rates is also interesting. The exit rates of all of the treatment and control groups were highest during the first 10 weeks following the claim filing date. After that point, the rates declined gradually, followed by a small increase approaching the point of exhaustion.

In summary, this analysis of survival rates and exit rates for UI claimants clearly supports the conclusion that the demonstration treatments reduced the weeks of UI payments received by

best copy available

FIGURE V.2
NUMBER OF WEEKS IN FIRST UI SPELL - EXIT RATES



claimants, with treatment 3 members showing the largest impact on weeks paid. The probability of remaining on UI was noticeably lower for all three treatment groups after the 5th week following the claim filing date, and the difference still existed at 26 weeks after that date.

D. IMPACTS OF THE TREATMENTS ON NONMONETARY DETERMINATIONS AND DENIALS

Each of the three treatments tested in the NJUIRDP required that claimants report to the demonstration office for orientation. Claimants were also expected to report for the remaining initial services--testing, the job-search workshop, and assessment/counseling--if they were scheduled to receive these services. Finally, following assessment, claimants were expected to report periodically to the demonstration office if they did not enter training or return to work.

Compliance with these reporting requirements was also monitored. Attendance was recorded by ES staff in the participant tracking system, which generated a weekly "delinquency report" that listed all claimants who failed to attend one of the initial scheduled events. This report was taken to the local UI office, and a claims examiner in each office was expected to "pend" the automated UI files for all individuals listed on the delinquency report. If these individuals claimed additional UI benefits, the pend indicator would not permit a payment to be made, and a fact- finding interview was triggered. The fact-finding interview and, if appropriate, a determination of eligibility were performed following NJ UI laws and regulations. Whether eligibility was denied depended on the reasons given during the fact-finding interview for failure to report for demonstration services, but claims examiners were instructed to inform claimants that failure to report without good cause could lead to benefit denial until the claimants reported and were rescheduled for services.

The procedures for monitoring claimants' compliance with the periodic reporting requirements following assessment were not as rigorous and systematic as those for the initial services. ES staff were expected to report any noncompliance to UI on an individual basis, but, as discussed in the "Implementation and Process Report," few instances of noncompliance were recorded in the tracking system.

These reporting requirements and the compliance process were expected to increase both the probability that a determination of eligibility would be made for nonseparation reasons and the probability that a denial would occur. As can be seen in Table V.6, increases did indeed occur, and the differences between each treatment group and the control group are statistically significant. On average, a nonmonetary determination was performed for 40 percent of the treatment group

TABLE V.6

NONSEPARATION ISSUE, NONMONETARY DETERMINATIONS, AND DENIALS

(Percent)

	Treatment Group			
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group
Number of Determinations				
0	59.1	59.2	62.1	72.0
1	25.8	26.5	24.6	18.9
2	9.4	9.3	7.9	6.0
3	4.3	3.5	3.6	2.2
4	1.4	1.6	1.8	1.0
Number of Denials				
0	80.5	81.5	82.4	85.5
1	14.4	14.0	13.2	11.1
2	4.2	3.3	3.1	2.3
3	0.6	0.9	1.1	0.7
4	0.3	0.3	0.2	0.3
Percent of Determinations Leading to a Denial	47.7	45.3	46.4	51.8
Sample Size	2,416	3,810	2,449	2,385

NOTE: The analysis is restricted to the first four nonmonetary determinations for any claimant during the current benefit year.

members, compared with 28 percent of the control group members; benefit denials were made for 19 percent of the treatment group members and 15 percent of the control group members. However, as these numbers imply, the probability that a determination led to a denial was lower for treatment members than for control group members (46 versus 52 percent). This finding suggests that the additional determinations that arose because of the demonstration reporting requirements yielded fewer denials than did a typical nonmonetary determination.

Further insight into the nonmonetary determination process in the demonstration can be obtained from the data presented in Table V.7 on the issues that were adjudicated. These data clearly show that the primary difference in the determinations of treatment and control group members was that those for treatment group members were more likely to involve reporting requirement issues than were those for controls. About one-quarter of the issues adjudicated for treatments were reporting issues, compared with 13 percent for controls. The distribution of nonseparation issues among the other major categories was similar for treatments and controls.

Data in the remainder of the table clearly show why the additional determinations performed on treatment members relative to control group members did not increase the overall denial rate as much as the increase in the determination rate. First, it was less likely that more than one issue was considered for treatment members than for control group members, thus reducing the probability of a denial for the treatment groups. And, second, data on the probability that consideration of a issue led to a denial show no significant treatment-control differences, except for the reporting requirement issue, the one issue that occurred more frequently for treatment members than for control group members. However, the probability that noncompliance with this requirement led to a denial was less than 25 percent for treatment members, compared with 44 percent for control group members, a statistically significant difference.

These findings suggest that claims examiners were reluctant to disqualify claimants for failing to report for services, if noncompliance with this requirement was the only issue raised in the determination. As noted in the process analysis, claims examiners did not deny benefits if claimants said that they had not received the mailed notice to report for orientation; instead, they referred claimants to orientation and denied benefits only when they did not attend this initial service.

One further question that can be raised about the nonmonetary determination and denial process is whether the additional determinations performed for treatment members were concentrated among individuals who did not participate in the various mandatory services. This question is addressed

TABLE V.7

NONMONETARY NONSEPARATION ISSUES AND THEIR OUTCOMES

(Percent)

	Treatment Group			
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group
Issues Considered				
Availability	20.6	19.1	18.2	21.4
Ability	6.5	5.4	6.6	7.2
Active search for work	3.4	3.5	5.0	4.8
Unsuitable work	1.4	1.5	1.6	2.2
Reporting requirements	24.9	26.9	26.1	13.0
Less than 7 days[a]	23.9	24.3	22.0	26.7
Pension offset	8.6	10.3	9.6	13.6
Other	10.8	9.2	10.9	11.3
Probability That More Than One Issue Is Considered	24.3	23.0	22.8	26.6
Probability of Denial[b]				
Availability	43.5	47.1	44.5	48.8
Ability	76.0	77.5	77.8	73.7
Active search for work	46.2	39.4	45.5	44.7
Unsuitable work	18.8	10.7	23.5	17.7
Reporting requirements	20.9	23.9	22.3	44.1
Less than 7 days	100.0	99.8	100.0	100.0
Pension offset[c]	1.0	0.5	0.0	0.9
Other	68.6	56.6	59.2	94.4
Sample Size (Number of Issues)	1,153	1,901	1,098	787

NOTE: The analysis is restricted to the first two nonmonetary determinations reported for each individual for the current benefit year. The data pertain to the first three issues reported for the first nonseparation issue determination.

[a] New Jersey's unemployment insurance program requires that claimants be eligible for the full calendar week to receive a benefit. When claimants are ineligible for any day during a calendar week, they are denied benefits for the full week. These issues are termed "less than 7 days."

[b] The probability of a denial is the percentage of determinations that lead to a denial.

[c] The pension offset determinations reported here reduce weekly UI benefits but do not lead to a denial. Thus, they are not shown as denials in Table V.5.

in Table V.8, which shows data on the likelihood of a determination or denial for treatment group members who collected more than 5 weeks of benefits. This sample restriction is used to focus that analysis on claimants who in most cases should have had a determination if they did not attend scheduled services. The data in the table clearly show that the probability of having a nonmonetary determination or denial was higher for claimants who did not attend orientation or other services than for those who did attend. However, the determination rates were roughly 50 percent for those who did not attend, suggesting that there may have been some slippage in the process of identifying and examining the eligibility of claimants who did not participate in the demonstration's mandatory services. However, further analysis of this issue indicates that the compliance process worked well. Among those who did not attend the scheduled orientation and continued to collect UI, approximately 90 percent either had a nonmonetary determination or were excused from the demonstration reporting requirements because they moved or had a gap in their period of UI collection.[17]

In summary, the increased determination and denial rates experienced by the treatment groups provide evidence that the compliance process was likely an important contributing factor to the impacts of the treatments on UI receipt. The increased denials probably had some direct negative impact on UI receipt although the indirect effects of the compliance process were probably more important.

---

[17] See further discussion in Chapter VIII of the "Implementation and Process Report."

TABLE V.8

NONSEPARATION ISSUE DETERMINATIONS AND DENIALS BY PARTICIPATION IN INITIAL SERVICES[a]

	Percent with a:	
	Determination	Denial
Orientation		
Attended	40.7	18.2
Did not attend	51.8	23.0
Job-Search Workshop		
Attended	40.9	18.5
Excused	37.0	14.8
Did not attend	51.2	24.0
Assessment/Counseling		
Completed	40.6	17.9
Did not complete	45.4	20.8
Sample Size	7,451	7,451

[a] The sample for these tabulations consists of treatment members who collected more than five weeks of benefits. The five-week restriction was used to focus the analysis on claimants who should have had determinations if they did not attend scheduled services.

VI. IMPACTS ON EMPLOYMENT AND EARNINGS

In general, the treatments in the NJUIRDP were expected to promote the rapid reemployment of claimants, and thus to have a positive impact on the employment and earnings of claimants following their entry into the UI system. Thus, a fundamental issue in the evaluation of the reemployment demonstration is whether such effects occurred. This chapter addresses this issue by examining the employment and earnings of claimants during the year following their initial UI claim. In addition, the characteristics of the first post-UI job are examined to determine whether the treatments had any effect on the types of jobs obtained by claimants. This is an important issue, since it is possible that, by promoting rapid reemployment, the treatments might have led claimants to accept jobs that were less desirable that those obtained by claimants who were not offered special services.

Section A of this chapter considers the general impacts of the services provided through the demonstration on the employment and earnings of UI claimants in the three treatments. The analysis also investigates the impacts of the treatments on demographic subgroups of the claimant population. Section B then evaluates the effect of the demonstration treatments on the length of time from the initial UI claim to reemployment. In the final section, we use data derived from the claimant interview to investigate the impact of the treatments on various characteristics of the first post-UI job, including a discussion of the wage rate received, the hours worked, and the occupation held in that job.

A. THE IMPACTS OF THE TREATMENTS ON EMPLOYMENT AND EARNINGS

In this section we examine impacts on employment and earnings. We begin by discussing the two sources of data available for this analysis--interview data and wage-records data. We then present our main estimates of the impacts of the three demonstration treatments on employment and earnings. These estimates are drawn from the interview data, since we believe that these data provide better estimates of the impacts of the treatments than do the wage-records data. However, we also present the wage-records impact estimates in the last subsection, since they provide an alternative set of impact estimates.

1. Data Sources and Issues

Two sources of data on employment and earnings were available and could have been used for this analysis. The first data source is the follow-up interview, which contains the responses of individuals to questions about their employment and earnings experience. The second data source consists of administrative data from the state wage-records files, which provide a measure of each claimant's earnings and weeks worked in UI-covered employment by calendar quarter. These data are reported by employers on a quarterly basis.

Both the interview data and the records data have drawbacks for the analysis. Data extracted from the interview responses fall prey to the same measurement error problems which plague any interview data set, such as faulty recall by respondents about the dates of employment and wage rates. Moreover, nonresponse to the survey could bias the results, although, as we show in Appendix B, nonresponse does not appear to be a serious problem with this survey. The wage-records data set, on the other hand, does not capture all earnings received by claimants. Employment found by workers outside the state of New Jersey[1] and employment outside the UI-covered sector, including self-employment,[2] are not included. The wage-records data set is also organized by calendar quarter, and more disaggregated time units cannot be used for analysis.

This latter situation presents, we believe, a major problem with using the wage-records data for this evaluation, since, as we showed in the previous chapter, much of the impact of the treatments appears to have occurred early in the claim spell when the treatments were introduced. One cannot use the wage-records data to focus very precisely on this time period, since the calendar quarter that contains the initial UI claim is likely to contain earnings data from the pre-UI job in addition to post-UI earnings. Moreover, the pre-UI earnings are likely to swamp any post-UI earnings, making it difficult to detect the impacts of the treatments. Since this problem can be avoided with the interview data by measuring the impacts from the date of the initial claim, we performed our main analysis based on the interview data. However, the wage records were used for a subsidiary analysis to provide an alternative set of impact estimates.[3]

2. Impacts on Employment and Earnings by Treatment Group

Our analysis of the impacts of the NJUIRDP on employment and earnings focused on two principal measures: (1) the proportion of time employed in each quarter following the claim filing date and (2) earnings per quarter. The probability of having a job during a quarter was also examined, but this measure of employment receives less attention in our discussion. These measures of employment and earnings were derived by using a respondent's reported beginning and ending dates and wage rates for each job held after the claim filing date.

---

[1] Employment outside the state may be important in the case of New Jersey because of the state's proximity to the large labor markets of the New York, Philadelphia, and Wilmington areas.

[2] About 3 percent of the eligible population reported on the interview that they were self-employed at some point after the claim filing date.

[3] A comparison of the wage records and interview data on earnings is presented in Appendix D.

The means for these employment and earnings measures are reported in Table VI.1 for the first four quarters following the claim filing date and for the entire year following that date.[4] The data on the proportion of time employed indicate that the claimants in the demonstration were employed for somewhat less than half of the year following the claim filing date. The quarterly figures show that rates of employment increased across time for each of the treatment and control groups, with the largest increase occurring between the first and second quarters. The earnings data show that the claimants earned approximately $8,000 to $9,000 over the year following their claim filing date. Earnings by quarter grew rapidly between the first and third quarters following the claim filing date, as did employment, with noticeable but more gradual increases in the fourth quarter.

The data in the table suggest that each of the treatments may have had positive effects on employment and earnings, since measured employment and earnings appear to be higher for the treatment group than for the control group. To examine these apparent impacts further and to provide estimates of these impacts, we used the regression model approach discussed in the previous chapter. We estimated models that used the quarterly values for the percent of time employed and earnings as the dependent variables, and used the same independent variables on claimant characteristics, office, and time as were used for the UI analysis. Both overall impacts and impacts by population subgroup were estimated.

In the next three subsections of this chapter, we use the estimated coefficients from these employment and earnings regressions to calculate the impacts of the treatments on the employment and earnings of UI claimants. Each of these subsections considers the impacts of one of the three treatments provided in the NJUIRDP--job-search assistance (JSA) only, JSA plus training and relocation, and JSA plus the reemployment bonus.

a. Job Search Assistance

The regression-based estimated impacts of job-search assistance (JSA) on employment and earnings are reported in Tables VI.2 and VI.3, respectively, for the first four quarters following the

---

[4] Later in this chapter, we examine quarters beyond a one-year period when we consider the effect of the JSA plus training or relocation treatment, which was expected to have longer-run impacts than the other treatments.

TABLE VI.1

EMPLOYMENT AND EARNINGS OUTCOMES BY TREATMENT GROUP:
SURVEY DATA

(Standard Errors of Means in Parentheses)

	Treatment Group			
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control
Mean Percent of Time Employed				
Quarter 1	16.6 (0.8)	16.1 (0.6)	17.1 (0.8)	14.2 (0.7)
Quarter 2	44.5 (1.4)	42.6 (1.1)	44.9 (1.4)	39.6 (1.2)
Quarter 3	60.6 (1.4)	58.6 (1.1)	59.1 (1.4)	56.2 (1.2)
Quarter 4	65.8 (1.5)	64.8 (1.2)	64.0 (1.4)	62.9 (1.3)
Year 1	47.1 (1.2)	45.3 (0.9)	45.8 (1.1)	42.8 (1.0)
Probability of Employment				
Quarter 1	34.2	33.7	36.1	30.3
Quarter 2	54.7	53.6	54.7	50.7
Quarter 3	67.3	65.7	66.2	64.9
Quarter 4	71.8	70.5	69.5	70.2
Year 1	73.8	73.7	72.1	72.6
Mean Earnings (dollars)				
Quarter 1	810 (50)	758 (37)	867 (51)	687 (40)
Quarter 2	2,189 (92)	2,014 (69)	2,270 (91)	1,945 (76)
Quarter 3	2,848 (97)	2,750 (75)	2,896 (96)	2,701 (85)
Quarter 4	3,021 (106)	3,048 (82)	3,097 (101)	3,012 (100)
Year 1	8,848 (327)	8,513 (244)	9,041 (314)	8,287 (282)
Sample Size	1,028	1,672	1,146	1,449

NOTE: The quarters are measured from the date of the initial UI claim.

TABLE VI.2

ESTIMATED IMPACT OF JSA ONLY ON THE PERCENT OF TIME EMPLOYED

(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Overall Impact	2.3** (1.1)	4.2*** (1.8)	4.3** (1.8)	2.8 (2.0)
Percent of Control Mean	16.2	12.4	7.7	4.5
Females	3.8	6.4	5.5	2.7
Males	1.3	3.9	3.1	2.1
Age ≥ 55 Years	1.2	3.7	7.5	7.5
Age 35 - 54 Years	4.0	6.5	4.4	1.3
Age 25-34 Years	1.0	3.8	1.5	1.4
Blacks	3.7	6.9	2.8	0.6
Whites	0.5	4.5	4.5	2.5
Other Races	6.9*	5.1	4.5	4.8
Union Hiring Hall Users	7.2	5.2	3.0	-1.4*
Did Not Use Union Hiring Hall	2.2	5.1	4.3	3.0
Expected Recall	5.6*	10.4*	10.8**	9.2**
Did Not Expect Recall	0.7	2.1	0.5	-0.9
Durable Manufacturing	0	1.4	6.5	3.6
Nondurable Manufacturing	3.6	10.4	9.3	6.5
Nonmanufacturing	2.8	4.5	1.9	1.2
Nongraduates	4.4	11.8	9.4	5.6
High School Graduates	2.8	3.4*	2.8	2.2
College Graduates	-0.5*	8.2	5.6	2.6

TABLE VI.2 (continued)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	3.5	5.4	6.0	4.9
No Prior Experience with UI	0.9	4.6	1.7	-0.4
Married, Spouse Employed	2.8	7.9	7.1	7.1*
Married, Spouse Not Employed	2.9	0.8	0.2	-3.0
Not Married	1.8	4.1	3.0	0.7
White Collar	5.1	8.8	8.3	7.9
Clerical	4.8	7.3	7.2	6.7
Blue Collar	0.4	2.5	1.2	-1.2
Quarter 1 Cohort	-0.6	4.1	1.9	-0.9
Quarter 2 Cohort	5.4	5.4	5.7	6.1
Quarter 3 Cohort	4.1	8.3	6.4	1.2
Quarter 4 Cohort	0.7	2.3	2.7	4.2

NOTE: The quarters are measured from the date of the initial UI claim. The impacts were estimated using the interview sample. (For the regressions, see Appendix D.) The statistical tests shown for the subgroup impacts test for differences within categories. For example, impacts for males are compared with impacts for females. For characteristics with more than two categories, the tests are, respectively, comparisons with ages 35 to 54, whites, nonmanufacturing, nongraduates, not married, blue collar and the quarter 4 cohort.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE VI.3

ESTIMATED IMPACT OF JSA ONLY ON EARNINGS
(Dollars)
(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Overall Impact	125** (63)	263** (111)	171 (116)	49 (129)
Percent of Control Group Mean	18.2	13.5	6.3	1.6
Females	119	157	94	-177
Males	141	357	233	142
Age ≥ 55 Years	71	40	16	-16
Age 35 - 54 Years	262	484	406	167
Age 25-34 Years	-36*	69	-107*	-291
Blacks	252	395	135	37
Whites	39	275	227	-40
Other Races	289	120	23	33
Union Hiring Hall Users	560	502	261	69
Did Not Use Union Hiring Hall	102	246	160	-16
Expected Recall	317*	622**	612**	381*
Did Not Expect Recall	26	60	-83	-230
Durable Manufacturing	58	208	498	410*
Nondurable Manufacturing	92	502	456	356
Nonmanufacturing	164	201	-24	-253
Nongraduates	240	608	450	235
High School Graduates	121	132	49	-51
College Graduates	115*	712	547	107

TABLE VI.3 (continued)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	159	220	205	92
No Prior Experience with UI	90	321	112	-156
Married, Spouse Employed	192	489*	372	219**
Married, Spouse Not Employed	67	-62	-158	-601
Not Married	94	172	103	39
White Collar	301*	369	103	-6
Clerical	285	584	694*	580*
Blue Collar	-4	104	26	-205
Quarter 1 Cohort	43	149	-107	-100
Quarter 2 Cohort	189	111	194	197
Quarter 3 Cohort	237	604	385	118
Quarter 4 Cohort	15	180	182	-274

NOTE: The quarters are measured from the date of the initial UI claim. The impacts were estimated using the interview sample. (For the regressions, see Appendix D.) The statistical tests shown for the subgroup impacts test for differences within categories. For example, impacts for males are compared with impacts for females. For characteristics with more than two categories, the tests are, respectively, comparisons with ages 35 to 54, whites, nonmanufacturing, nongraduates, not married, blue collar and the quarter 4 cohort.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

claim filing date. The estimated impacts of the treatment on the proportion of time employed indicate that claimants who received JSA worked a greater percentage of time on average during each of the first four quarters following the claim filing date than did claimants in the control group, and these estimated impacts are statistically significant for the first three quarters. During the first quarter, the impact was 2.3 percentage points, which represents about a 16 percent increase in employment relative to the control group mean. In subsequent quarters, the point estimates are higher, but they represent a smaller percentage of the control group mean. Thus, it appears that this treatment was effective at encouraging rapid reemployment, which was its intention. Because it was not intended to promote long-term employment and earnings prospects, it is thus not surprising that the impact declined over time. These findings are consistent with those found with the UI data, which suggested that job-search assistance had an impact early in the unemployment spells of claimants.

The earnings impacts reported in Table VI.3 are, as one would expect, consistent with the employment impacts. They demonstrate that job-search assistance increased the earnings received by claimants following the claim filing date. Claimants who were offered job-search assistance earned $125 more in the first quarter, on average, than did claimants in the control group, an increase of approximately 18 percent. Job-search assistance significantly increased earnings for claimants in the first and second quarters, while the estimated impacts for the third and fourth quarters were positive but not statistically significant. Like the estimated impacts on employment, the impacts on earnings declined over time.

The impacts of job-search assistance on quarterly earnings in percentage terms were nearly equal to the employment impacts for the corresponding quarters. Thus, the earnings impacts appear to have been driven primarily by the impacts on employment, rather than by impacts on hourly wage rates (the impacts of the treatments on wage rates are considered explicitly later in this chapter).

Tables VI.2 and VI.3 also report the estimated impacts of job-search assistance on the employment and earnings of demographic subgroups of the claimant population, indicating significant differences between the subgroup impacts. Although few of the differences between subgroups were significant (in part, because of small sample sizes), some of the results are worth noting:

- Impacts for claimants who expected to be recalled were greater than for those who did not.
- Impacts for clerical workers and white-collar workers exceeded those for blue-collar workers.
- Impacts were greater for nongraduates than for college or high school graduates.

- Impacts, particularly on earnings, were larger for the middle-age group than for the younger or older age groups.
- Impacts by industry were greatest for nondurable goods manufacturing.

Only the greater impact for workers who expected to be recalled was consistently significant over time.

These subgroup findings are generally consistent with the estimated impacts on UI receipt by subgroup that were presented in Chapter V. Claimants with marketable skills, such as clerical workers and white-collar workers, appear to have experienced greater impacts than did individuals who faced long-term, structural unemployment problems, such as blue-collar workers, workers from durable-goods manufacturing industries, and permanently separated workers. This conclusion implies that the displaced with more severe reemployment problems may have been less affected by this treatment than were workers who faced relatively more favorable reemployment prospects.

One subgroup finding that requires further investigation is that individuals who expected to be recalled were affected by the demonstration treatments. This may seem counter-intuitive, since recall decisions are made by employers, not by claimants. Thus, the mechanism by which this treatment increased employment rates for this group is not readily apparent. The answer appears to pertain to the fact that not all individuals who expect to be recalled but who do not have a definite date of recall (individuals with a definite date of recall were not demonstration-eligible) are actually recalled. Among the control group, this percentage was 50.3 percent; for treatment 1, it was 46.4 percent. Moreover, the percentage of treatment 1 claimants who expected to be recalled and who did not have a job as of the interview date was lower than the percentage observed for the control group (15.5, compared with 18.3 percent, respectively). Thus, this treatment appears to have increased employment among those who expected to be recalled by increasing the probability that they would accept a new job as opposed to waiting until they were recalled.[5]

b. JSA Plus Training and Relocation

Our expectations about the impact of the JSA plus training or relocation treatment on employment and earnings were less certain than for the other treatments. Individuals who did not enter training were expected to experience increased employment and earnings because of the job-search assistance provided by the treatment. However, individuals who enrolled in training were not expected to experience such impacts in the short-run, since they remained unemployed for the duration of their training. The benefits of training were expected to appear eventually as enhanced employment stability and possibly increased earnings.

---

[5] Data on the amount of time spent looking for a job do not show that the treatment affected the intensity of job search among those who expected to be recalled; however, it may have affected the likelihood that they would accept a job offer.

Tables IV.4 and VI.5 present the impacts of this treatment on employment and earnings. As can be seen, the impacts were positive in each quarter, but smaller than the impacts for job-search assistance alone that were discussed in the previous section. For this reason, fewer of the estimated impacts are statistically significant (the impacts for the first two quarters for the percent of time employed are significant). In addition, as we found for the impacts of JSA, the impacts of treatment 2 decreased over time--for example, decreasing for the percent of time employed from a maximum of 13.4 percent of the control group mean to a minimum of 2.7 percent in the fourth quarter.

Tables VI.4 and VI.5 also show that, for the most part, the impact of treatment 2 did not vary significantly by demographic subgroup. One exception was the education subgroups, where treatment 2 increased employment more among high school graduates and nongraduates than among college graduates. A similar distinction between earnings impacts existed for these subgroups, though it was not statistically significant. The subgroup impacts of treatment 2 thus provide us with little additional information.

An important question about treatment 2 is whether the provision of training to some individuals in this treatment had a positive effect on their employment and earnings. As we noted above, impacts on employment and earnings were generally larger for the JSA-only treatment than for the JSA plus training or relocation treatment throughout the first four quarters after the claim filing date,[6] so we can conclude that the additional offer of training did not have a positive impact on employment and earnings over this period. However, since we expected that the impact of training would occur only in the longer run after training was completed, this finding does not mean that training had no impacts. To investigate the possibility of longer-run impacts, we examined earnings in quarters 5 and 6.[7] This investigation also showed no impacts (the point estimates for earnings were $2 in quarter 5 and -$124 in quarter 6, and neither impact was statistically significant). However, this finding should also not

---

[6] There is one exception--the point estimate for the treatment 2 impact on earnings in the fourth quarter was larger than the treatment 1 impact--but in that instance the impacts for both treatments were small and not statistically significant.

[7] Sample sizes for quarters 5 and 6 were substantially smaller than for quarters 1 to 4. The sample size (treatments plus controls) was 3,033 for quarter 5, about 60 percent of the sample available for the analysis of quarters 1 to 4, and 1,301 for quarter 6, about 25 percent of the sample available for analysis of quarters 1 to 4.

TABLE VI.4

ESTIMATED IMPACT OF JSA PLUS TRAINING/RELOCATION ON THE PERCENT OF TIME EMPLOYED

(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Overall Impact	1.9** (0.9)	2.8* (1.6)	2.2 (1.6)	1.7 (1.7)
Percent of Control Group Mean	13.4	7.0	3.9	2.7
Females	3.0	5.8	4.8	2.5
Males	1.4	0.8	0.5	1.4
Age ≥ 55 Years	1.9	7.3	6.8	1.8
Age 35 - 54 Years	2.3	1.9	0.8	1.0
Age 25-34 Years	2.1	2.2	2.4	3.3
Blacks	3.9	8.9*	0.6	3.0
Whites	0.7	0.4	1.4	-0.6
Other Races	4.8	6.4	7.4	8.0
Union Hiring Hall Users	8.5	4.1	0.6	-10.7*
Did Not Use Union Hiring Hall	1.7	3.1	2.7	2.7
Expected Recall	3.8	5.8	2.1	1.8
Did Not Expect Recall	1.3	1.7	2.8	2.0
Durable Manufacturing	1.4	0.2	0.6	1.6
Nondurable Manufacturing	1.4	6.2	3.3	3.8
Nonmanufacturing	2.6	3.1	3.0	1.4
Nongraduates	3.6	3.4	-1.9	-1.2
High School Graduates	2.8	4.7	4.9	2.9
College Graduates	-1.7**	-4.5*	-5.9	-0.6

TABLE VI.4 (continued)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	2.8	3.7	3.8	4.0
No Prior Experience with UI	1.2	2.4	0.8	-1.0
Married, Spouse Employed	2.5	2.4	1.4	2.4
Married, Spouse Not Employed	1.5	0.8	0.3	1.4
Not Married	2.1	5.3	5.2	1.6
White Collar	4.1	6.9	4.9	2.9
Clerical	3.1	1.5	-1.6	4.9
Blue Collar	0.9	1.9	2.8	0.4
Quarter 1 Cohort	1.1	1.1	-1.7	-1.6
Quarter 2 Cohort	2.7	6.9	6.4	4.8
Quarter 3 Cohort	1.3	2.5	4.2	3.4
Quarter 4 Cohort	3.2	1.6	0.4	0.3

NOTE: The quarters are measured from the date of the initial UI claim. The impacts were estimated using the interview sample. (For the regressions, see Appendix D.) The statistical tests shown for the subgroup impacts test for differences within categories. For example, impacts for males are compared with impacts for females. For characteristics with more than two categories, the tests are, respectively, comparisons with ages 35 to 54, whites, nonmanufacturing, nongraduates, not married, blue collar and the quarter 4 cohort.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE VI.5

ESTIMATED IMPACT OF JSA PLUS TRAINING/RELOCATION
ON EARNINGS
(Dollars)
(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Overall Impact	82 (55)	103 (98)	83 (103)	77 (112)
Percent of Control Group Mean	11.9	5.3	3.1	2.6
Females	121	185	223	2
Males	75	64	-14	51
Age ≥ 55 Years	28	120	134	-86
Age 35 - 54 Years	118	110	78	77
Age 25-34 Years	113	142	107	33
Blacks	174	147	-175	-133
Whites	40	39	86	-44
Other Races	196	332	353	354
Union Hiring Hall Users	461	216	101	-393
Did Not Use Union Hiring Hall	73	116	99	56
Expected Recall	162	276	37	-34
Did Not Expect Recall	60	35	134	63
Durable Manufacturing	74	10	211	374
Nondurable Manufacturing	21	108	58	176
Nonmanufacturing	128	159	79	-122
Nongraduates	166	168	-56	-43
High School Graduates	112	162	142	44
College Graduates	-5	-35	68	162

TABLE VI.5 (continued)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	142	130	156	103
No Prior Experience with UI	33	109	18	-79
Married, Spouse Employed	134	175	135	96
Married, Spouse Not Employed	99	60	-40	-293
Not Married	53	93	132	120
White Collar	152	225	14	-29
Clerical	84	-8	-65	140
Blue Collar	74	113	195	19
Quarter 1 Cohort	18	30	-236	-210
Quarter 2 Cohort	180	293	325	330*
Quarter 3 Cohort	65	172	267	237
Quarter 4 Cohort	94	-30	-27	-339

NOTE: The quarters are measured from the date of the initial UI claim. The impacts were estimated using the interview sample. (For the regressions, see Appendix D.) The statistical tests shown for the subgroup impacts test for differences within categories. For example, impacts for males are compared with impacts for females. For characteristics with more than two categories, the tests are, respectively, comparisons with ages 35 to 54, whites, nonmanufacturing, nongraduates, not married, blue collar and the quarter 4 cohort.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

be considered conclusive, since relatively few individuals in this treatment were placed in training, and it is thus difficult to detect the impacts of training unless they are quite large.[8] Moreover, the data from the interview indicated that about 14 percent of the individuals who received classroom training were still unemployed at the time of the interview, in part because they had only recently completed training or were still in training. Thus, a longer time period may be necessary to measure any impacts of training.[9]

c. JSA Plus the Reemployment Bonus

Treatment 3 combined job-search assistance with the offer of a reemployment bonus to workers who found jobs within 11 weeks after the offer was made. We expected that treatment 3 would have the greatest impact on employment because of the additional reemployment incentives created by the bonus offer. We were less certain about its impacts on earnings, because the incentives created by the bonus might have led workers to sacrifice earnings in an effort to find a job quickly and collect the bonus, especially given that the bonus declined substantially over time.[10] The impacts of the combination of job-search assistance and the reemployment bonus offer on employment and earnings are reported in Tables VI.6 and VI.7. These estimates show that treatment 3 increased the percentage of time employed in quarters 1 and 2 by 19.7 and 12.6 percent, respectively. Earnings increased by 23.3 and 14.3 percent in the two quarters. These impacts were larger than those found for job-search assistance alone, and they are significantly different from zero at the 99 percent confidence level. The impacts of treatment 3 in the last two quarters declined rather sharply and are not statistically significant. In addition, these impacts were somewhat smaller than those estimated for job-search assistance alone.

Several differences by demographic subgroup existed for impacts on both employment and earnings in the first two quarters following the claim filing date:

- o Impacts on the earnings of males were greater than impacts on the earnings of females in quarter 1.

---

[8] In addition, about 16 percent of the individuals who entered classroom training did not complete training. The two primary reasons cited by respondents for not completing training were that they became reemployed and that they could not afford to continue.

[9] Data from the interview indicate that about two-thirds of the claimants who received classroom training, and who had completed it long enough ago to have an opinion, felt that it had been useful to them in finding a job.

[10] In Section C of this chapter, we address the nature of the jobs obtained through this treatment.

TABLE VI.6

ESTIMATED IMPACT OF JSA PLUS REEMPLOYMENT BONUS ON THE PERCENT OF TIME EMPLOYED

(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Overall Impact	2.8***	5.0***	2.3	0.6
	(1.0)	(1.7)	(1.8)	(1.9)
Percent of Control Mean	19.7	12.6	4.1	1.0
Females	1.2	5.3	3.5	1.6
Males	4.1	3.7	0.5	-1.2
Age $\geq$ 55 Years	1.0*	3.1	3.4	2.3
Age 35 - 54 Years	5.7	8.7	4.2	-0
Age 25-34 Years	-0.7**	-1.5**	-2.7	-1.2
Blacks	5.7	11.3	3.6	-0.9
Whites	1.9	2.4	0.5	-1.6
Other Races	2.6	4.7	4.6	6.0
Union Hiring Hall Users	3.5	5.8	-2.1	-9.8
Did Not Use Union Hiring Hall	2.7	4.4	2.2	0.8
Expected Recall	3.7	8.0	5.8	2.3
Did Not Expect Recall	2.2	2.5	-0.2	-1.1
Durable Manufacturing	1.0	-2.8	-3.0	-1.5
Nondurable Manufacturing	0.2*	11.5*	5.3	4.1
Nonmanufacturing	1.7	4.0	1.9	-1.6
Nongraduates	-0.1	2.9	-0.5	-2.0
High School Graduates	5.5**	6.5	3.9	1.3
College Graduates	-8.3***	-4.2	-6.7	-3.8

TABLE VI.6 (continued)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	2.5	3.1	1.0	1.8
No Prior Experience with UI	3.0	6.3	3.3	-2.1
Married, Spouse Employed	0.5	0.4*	-0.5	-1.3
Married, Spouse Not Employed	3.6	9.6	6.6	3.2
Not Married	4.9	6.4	2.2	0.2
White Collar	4.9	8.1	4.6	3.3
Clerical	7.4**	3.7	-0.7	-1.9
Blue Collar	0.2	2.9	1.5	-0.7
Quarter 1 Cohort	3.2	6.7	1.5	-1.4
Quarter 2 Cohort	5.2	10.5*	8.7*	8.4
Quarter 3 Cohort	0.5	-0.6	-0.9	-4.1
Quarter 4 Cohort	2.5	2.3	-0.9	-1.4

NOTE: The quarters are measured from the date of the initial UI claim. The impacts were estimated using the interview sample. (For the regressions, see Appendix D.) The statistical tests shown for the subgroup impacts test for differences within categories. For example, impacts for males are compared with impacts for females. For characteristics with more than two categories, the tests are, respectively, comparisons with ages 35 to 54, whites, nonmanufacturing, nongraduates, not married, blue collar and the quarter 4 cohort.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE VI.7

ESTIMATED IMPACT OF JSA PLUS REEMPLOYMENT BONUS ON EARNINGS
(Dollars)
(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Overall Impact	160*** (60)	278*** (107)	131 (113)	22 (124)
Percent of Control Mean	23.3	14.3	4.9	0.7
Females	33	144	147	-54
Males	261*	310	51	-73
Age ≥ 55 Years	56	150	46	-140
Age 35 - 54 Years	268	435	235	38
Age 25-34 Years	37	-36*	-88	-171
Blacks	206	398	-5	-246
Whites	153	174	77	-89
Other Races	108	258	231	149
Union Hiring Hall Users	179	220	-77	-436
Did Not Use Union Hiring Hall	150	232	108	-39
Expected Recall	283	477	353	190
Did Not Expect Recall	78	93	-47	-206
Durable Manufacturing	47	-210	-211	-109
Nondurable Manufacturing	-170*	152	-59	95
Nonmanufacturing	99	230	116	-188
Nongraduates	-54	-33	-311	-275
High School Graduates	226*	277	175	9
College Graduates	10	356	129	-88

TABLE VI.7 (continued)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	149	124	21	-22
No Prior Experience with UI	156	383	204	-123
Married, Spouse Employed	5	-55**	-138*	-213
Married, Spouse Not Employed	283	638	477	-46
Not Married	250	342	164	98
White Collar	220	341	28	-86
Clerical	403*	302	23	-166
Blue Collar	37	153	154	-20
Quarter 1 Cohort	186	296	-143	-192
Quarter 2 Cohort	402	622*	533	544**
Quarter 3 Cohort	-96	-22	59	-312
Quarter 4 Cohort	128	67	23	-322

NOTE: The quarters are measured from the date of the initial UI claim. The impacts were estimated using the interview sample. (For the regressions, see Appendix D.) The statistical tests shown for the subgroup impacts test for differences within categories. For example, impacts for males are compared with impacts for females. For characteristics with more than two categories, the tests are, respectively, comparisons with ages 35 to 54, whites, nonmanufacturing, nongraduates, not married, blue collar and the quarter 4 cohort.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

- The impacts on employment and earnings for middle-age claimants exceeded those for the younger claimants.

- High school graduates were affected more by treatment 3 than were nongraduates and college graduates.

- The impacts of the treatment were lower for claimants with an employed spouse than for married claimants whose spouse was not employed.

- The impacts for clerical workers were greater than the impacts for blue-collar workers.

The subgroup impacts found here were similar to those found for job-search assistance. The estimated subgroup impacts for treatment 3 thus support the conclusion outlined in our discussion of the JSA-only impacts that claimants with marketable skills appear to have been affected by the treatments to a greater extent than were workers who faced more severe long-term, structural unemployment problems.

To evaluate the impact of the reemployment bonus component of treatment 3, we compared claimants in treatment 1 who were assessed with those in treatment 3 who were assessed, since the bonus offer was made at the time of assessment. The results of making this comparison are reported in Table VI.8. As can be seen, the impacts of the bonus offer on employment and earnings are not as strong as the estimated impacts on UI receipt; only the impact on earnings in the first quarter after the date of claim is statistically significant at the 90 percent confidence level.[11] Nevertheless, there is some weak evidence that the bonus offer led to increased earnings.

3. Impacts of the Treatments Based on Wage-Records Data

Wage-records data are used in this section to provide an alternative estimate of the impacts of the treatments on earnings over the year following the claim filing date. As noted earlier, the wage-records data have one serious shortcoming for this analysis--namely, they are available only by calendar quarter, and, for this reason, they cannot be disaggregated to examine the impacts that began with the entry of claimants into the UI system. This presents a problem for the analysis. If we use the calendar quarter that contains the date of the claim, both some pre-UI and post-UI earnings will be included. While random assignment to treatment and control groups should make pre-UI earnings equal among groups, the fact that pre-UI earnings are likely to exceed any post-UI earnings in this quarter means that any impacts of the treatments are likely to be swamped by statistically insignificant differences in pre-UI earnings. This, in fact, appears to occur. No significant differences among treatment

---

[11] This difference in results may arise because the variance of earnings is larger relative to its mean than the variance of UI receipt relative to its mean. As a result, statistically significant differences in earnings are harder to detect than differences in UI receipt, given a fixed sample size.

TABLE VI.8

ESTIMATED IMPACTS ON EMPLOYMENT AND EARNINGS OF OFFER OF REEMPLOYMENT BONUS ON CLAIMANTS WHO WERE OFFERED THE BONUS

(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Percent of Time Employed	0.7	1.4	-1.8	-2.8
	(0.7)	(2.0)	(2.2)	(2.3)
Earnings (Dollars)	58*	90	-22	-58
	(33)	(113)	(137)	(149)

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

and control groups appear for this quarter. Thus, the analysis presented herein examines the impacts that began with the first calendar quarter following the claim filing date, but this analysis is likely to miss any impacts that occurred early in the unemployment spell. Since our UI analysis indicated that such impacts did occur, we believe that the wage-records-based impact estimates are lower-bound estimates of the impacts of the treatments, and that the interview-based estimates present a better picture of the impacts.[12]

The impacts on earnings that are estimated on the basis of wage-records data are reported in Table VI.9 for quarters beginning with the first calendar quarter after the claim filing date. As can be seen, the estimate for treatment 3 for the first quarter is statistically significant, and it is quite similar in magnitude to the estimate obtained from the interview data. However, all remaining estimates are considerably lower than we found using the interview data, and none is statistically significant. Thus, these data do not generally support our previous finding that each of the treatments had an impact on employment and earnings. Nevertheless, the point estimates of impacts are positive when summed over the year, and we use these as lower-bound estimates of the impacts of the treatments on earnings in our benefit-cost analysis.

B. THE IMPACTS OF THE TREATMENTS ON TIME TO REEMPLOYMENT

In the previous section of this chapter, we investigated the impacts of the treatments on the employment and earnings of UI claimants. Although we described in detail the extent of these impacts, we did not attempt to determine how the observed impacts occurred. This section deals with one possible source of treatment impacts by considering how the duration of a worker's initial spell of not working was affected by the treatment which he or she was offered.[13] We use the initial spell of not working as opposed to the unemployment spell, since it is difficult to determine whether and when individuals in the sample dropped out of the labor force.

Our analysis of the impacts of the treatments on the initial spell of not working was performed in two ways. First, we used our basic regression model with the spell length as the dependent

---

[12] Estimated impacts using the wage records data are also biased downward since the data were only available for New Jersey. Earnings outside the state were not obtained.

[13] We used the time from the initial UI claim to the first post-UI job as our measure of this spell, rather that the time from the end of the pre-UI job to the first post-UI job. We did so both because we believed that the end date of the pre-UI job was subject to more measurement error than was the UI claim filing date and because we could compare the timing of the intervention with the spell by using the claim filing date. However, both measures were computed, and they both provided similar results.

TABLE VI.9

ESTIMATED IMPACTS OF TREATMENTS ON EARNINGS: WAGE RECORDS DATA

(Dollars)
(Standard Errors in Parentheses)

Calendar Quarters Following the Date of Claim	Treatment Group		
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus
Quarter 1	28 (83)	58 (75)	176** (83)
Quarter 2	75 (90)	-23 (81)	79 (89)
Quarter 3	98 (82)	47 (75)	52 (82)
Quarter 4	-39 (93)	-10 (83)	16 (92)

*Statistically significant at the 90 percent confidence level for a two-tailed test.

**Statistically significant at the 95 percent confidence level for a two-tailed test.

***Statistically significant at the 99 percent confidence level for a two-tailed test.

variable to estimate the impacts of the treatments. This approach (see Table VI.10) suggests that each of the treatments reduced the time between the UI claim filing date and reemployment. Respondents from treatment group 1 averaged 2.1 weeks less in the initial period of not working than did respondents from the control group--a difference which represents about 7 percent of the mean weeks of duration for the control group (31 weeks), and which is statistically significant at the 95 percent confidence level. The estimates for the other two treatment groups showed an approximately one-week reduction in duration, but neither result was statistically significant. These estimates should be viewed with caution, however, since the full spell length was not observed for individuals who were still not working as of the interview date (which occurred for about 20 percent of the sample). This censoring of the spell length may bias these estimates of the impacts on duration, particularly if, as is the case, the censoring was more severe for the control group than for the treatment groups. This situation should lead to an underestimate of the true effects of the treatments.

Because of this problem, a second approach, which addresses the problem of censored observations of duration, was adopted for this analysis. This approach, which in recent years has come to be used widely in analyses of unemployment duration (see discussion in Kiefer, 1988), entails computing weekly rates of exit from the not-working status to employment. The distributions of these exit rates can be compared to determine whether they differ for different sample groups. A closely related measure is the survival rate, which measures at each point in time the probability that a randomly chosen respondent from a given sample is not working past that point in time.[14]

The estimated weekly survival rates for each treatment group are presented in Figure VI.1. As can be seen, the control group appears to have had the highest survival rate for all weeks during the first year following the claim filing date, implying that a greater proportion of the workers in the control group were still not working at any point in time relative to the three treatment groups. The results of statistical tests of the differences between the estimated survival functions for treatment and control samples were mixed,[15] but the results were strong enough to suggest that the treatments were effective at reducing the time between the UI claim filing date and reemployment.

---

[14] This methodology was also used to analyze UI duration. (See Chapter V for references and further discussion.)

[15] We used both the log rank tests of homogeneity of exit behavior between treatment groups and the Wilcoxon test. The log rank tests did not show statistically significant differences between groups, while the Wilcoxon tests did.

TABLE VI.10

ESTIMATED IMPACTS OF TREATMENTS ON TIME TO REEMPLOYMENT

(Standard Errors in Parentheses)

	Treatment Groups		
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus
Impact on Not Working Duration (Weeks)	-2.13** (0.97)	-0.86 (0.85)	-0.96 (0.93)

*Statistically significant at the 90 percent confidence level for a two-tailed test.

**Statistically significant at the 95 percent confidence level for a two-tailed test.

***Statistically significant at the 99 percent confidence level for a two-tailed test.

Comparisons of the survival-rate plots in Figure VI.1 by treatment group show some differences among the treatments. For example, treatment 2 appears to show smaller impacts relative to the control group over the first half of the year than is true of the other treatments. However, since most of the differences among treatments appear in the tail of the distribution (where sample sizes are small), we should not attempt to draw too many conclusions from these differences. Instead, it is perhaps more interesting to examine the exit rates shown in Figure VI.2. While it is difficult to interpret these plots since they jump around to a large extent, two points are worth mentioning. First, the impact of the treatments that we observed early in the unemployment spells of claimants based on the UI data (see Chapter V) is also apparent in Figure VI.2. That is, in general, the exit rates for the treatments appear to be higher than those for the control group over the first 12 to 16 weeks after the claim filing date. Second, the exit rates for both treatments and controls are high both initially and again in the 26- to 29-week range. The first peak period represents a time at which a substantial portion of the sample was able and willing to find a job quickly. The second peak period is likely to be caused by the exhaustion of UI benefits, a point at which many workers must find a new job to avoid the severe financial hardship from the loss of benefits, and some workers take jobs which they avoided while UI benefits were still available.

The results summarized in this section imply that the impacts of the treatments on the claimants' initial spells of not working were probably responsible for the majority of the impacts of the treatments on employment and earnings which were discussed earlier. The impacts for at least treatment 1 were relatively large, reducing initial spells of not working by as much as two weeks or more, and statistically significant. The magnitude of this impact is quite consistent with the employment impacts cited earlier. For example, the estimates in Table VI.2 aggregated over the entire year indicate that the estimated impact of treatment 1 was to increase the time of employment by 3.6 percent. Multiplying this impact by 52 weeks yields an impact of 1.9 measured in weeks. This result is close to the least squares estimate of the impact of treatment 1 on unemployment duration presented in Table VI.10.[16] While the consistency between these results is not surprising, given that both employment measures were derived from the same survey responses, it does demonstrate that it was primarily the initial spell of unemployment duration which was affected by the treatment. It is also comforting to know that we derived similar employment impacts for the demonstration treatments when we used different dimensions of measurement and methods of estimation.

---

[16] Performing the same calculations for the other treatment groups yields a similar conclusion.

FIGURE VI.1
DURATION TO FIRST POST-UI JOB - SURVIVAL RATES

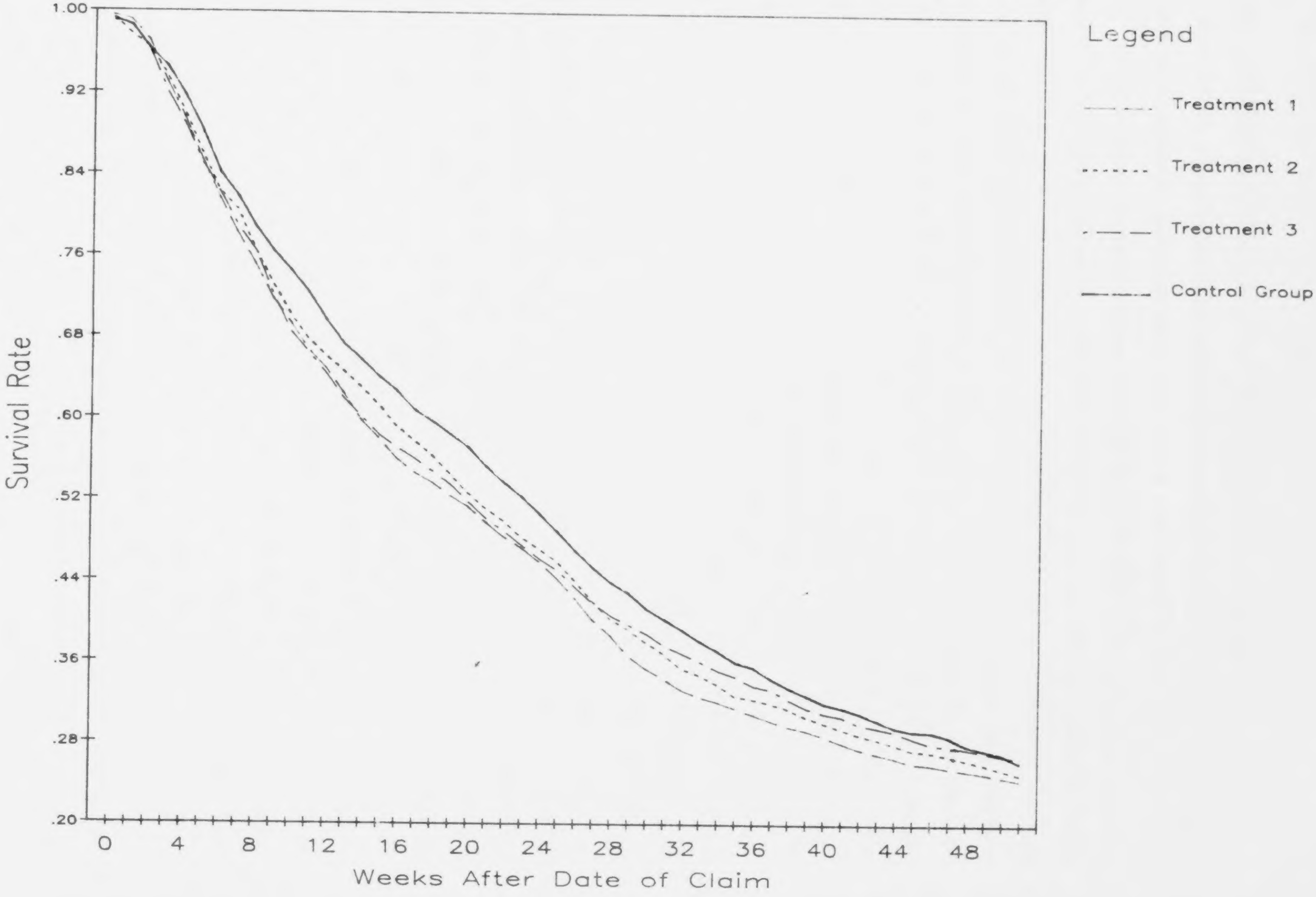
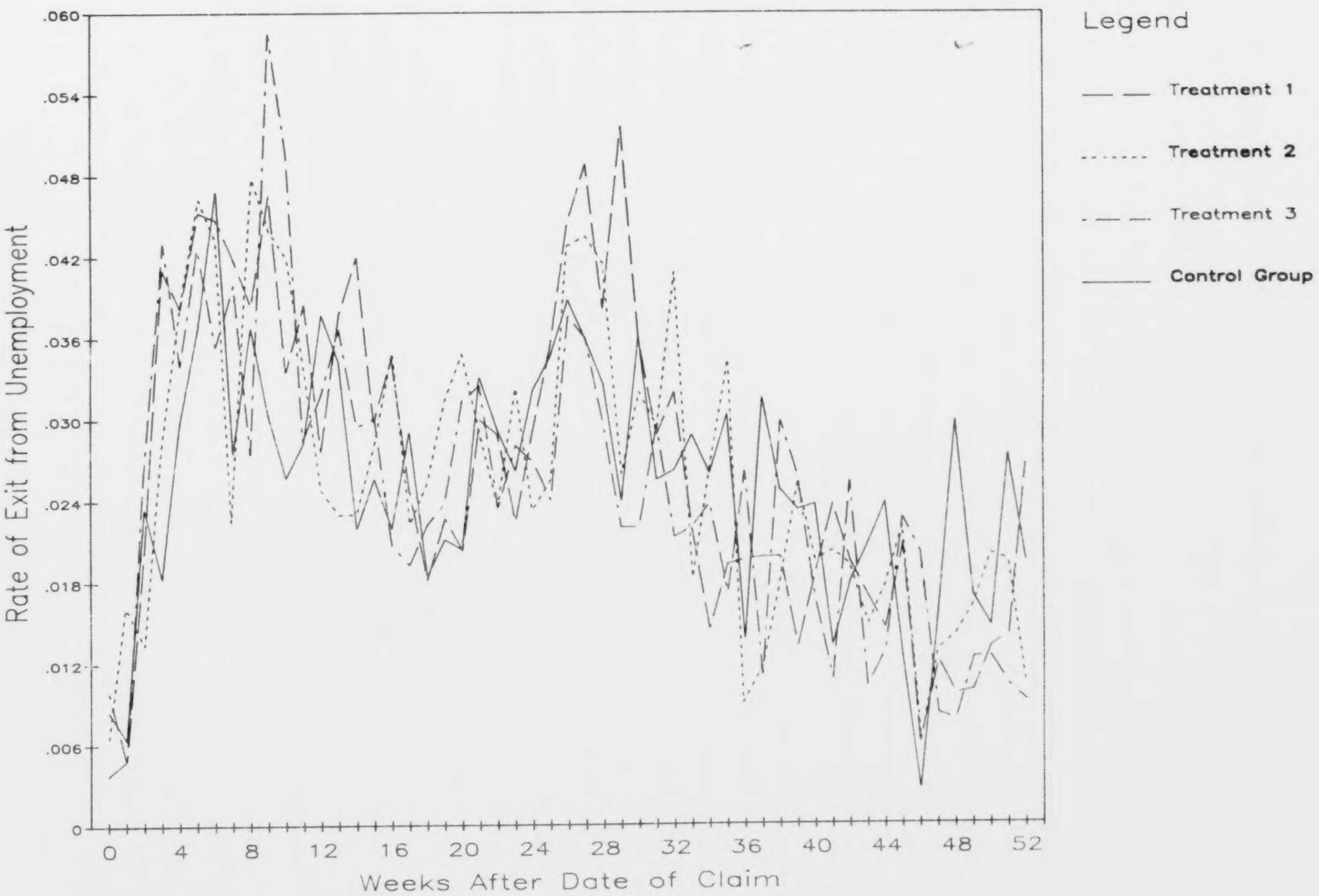


FIGURE VI.2
DURATION TO FIRST POST-UI JOB - EXIT RATES



C. IMPACTS OF THE TREATMENTS ON THE FIRST POST-UNEMPLOYMENT JOB

Earlier sections of this chapter have demonstrated that the program treatments appear to have been effective at encouraging the reemployment of UI claimants. The possibility exists, especially for the reemployment bonus treatment, that the claimants in the treatment groups who returned to the work force quickly sacrificed earnings on their jobs to hasten their reemployment. For example, job-search assistance may have encouraged claimants to move to lower-paying occupations in which openings could be found more readily. Workers who were offered the reemployment bonus in treatment group 3 may also have taken a low-paying job to obtain a substantial reemployment bonus. However, job-search assistance could also have improved the job-search skills of claimants, and they may have found better jobs than they would have otherwise. These issues are addressed in this section by examining hourly wage rates and hours worked for those employed. Changes in occupations are also addressed.

1. Post-UI Hourly Wage Rates

Table VI.11 presents information on the mean pre- and post-UI hourly wage rates for individuals who became reemployed.[17] The mean pre- and post-UI wage rates declined for each of the treatment and control groups, indicating that many claimants experienced some decline in hourly wage rates due to their unemployment spell. Moreover, the decline in mean hourly wage rates was largest for the control group, suggesting that the treatments did not have an adverse effect on post-UI hourly wage rates. In fact, the treatments may have had a positive effect on post-UI hourly wages, since the decline in mean hourly wages was smaller for members of the treatment groups than for members of the control group.

To investigate whether the treatments had a positive effect on post-UI hourly wages, we estimated two regression models. One model used the post-UI wage rate as the dependent variable, and the other used the natural log of the wage rate. The natural log specification is often used in the economics literature for wage equations. The results of this exercise are also reported in Table VI.11. As can be seen, the estimated impacts were positive for all treatment groups, and they are statistically significant when the natural log is used (the impact of treatment 1 is also statistically significant when the dollar value of the wage rate is used). The coefficients for the natural log specification can be interpreted as the percentage change in the post-UI wage rate brought about by the treatments.

---

[17] Hourly wage rates rather than weekly earnings are used in this analysis in order to compare compensation levels more accurately across jobs for a constant amount of effort.

TABLE VI.11

PRE- AND POST-UI HOURLY WAGES
(Dollars)

(Standard Errors in Parentheses)

	Treatment Group			
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group
Means				
Pre-UI hourly wage	9.56 (0.18)	9.45 (0.14)	9.89 (0.16)	9.76 (0.16)
Post-UI hourly wage	9.49 (0.21)	9.21 (0.15)	9.58 (0.17)	9.22 (0.16)
Difference	-.07	-.24	-.31	-.54
Estimated Treatment Impact				
Post-UI wage rate	0.37* (0.20)	0.19 (0.18)	0.26 (0.20)	
Natural log of post-UI wage rate	0.041** (0.017)	0.030** (0.015)	0.041** (0.017)	
Sample Size	819	1,347	898	1,138

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

Thus, the estimated impacts are that treatments 1 and 3 increased hourly wage rates by 4 percent and treatment 2 by 3 percent.

In summary, our examination of hourly wage rates on the post-UI job shows no evidence that claimants in the treatment groups may have sacrificed wages to become reemployed quickly. Instead, there is some evidence that each of the treatments led to increased hourly wage rates on the post-UI job.

2. Work Hours in the Post-UI Job

Another dimension of post-UI jobs that may have been affected by the treatments is hours worked on the post-UI job. Although the treatments did not cause a decline in wage rates, it is possible that claimants who were offered the treatments may have sacrificed earnings by accepting a part-time job to facilitate reemployment.[18] Alternatively, by providing job-search assistance, the treatments may have enabled treatment group members to find full-time employment more readily than could members of the control group.

Data to investigate these possibilities are reported in Table VI.12. These data demonstrate that, in general, claimants worked fewer hours per week in their post-UI job relative to their pre-UI job. The decline was about 2 hours, from an average of 41 or 42 hours per week to an average of 39 or 40. However, the treatments did not reduce the mean number of hours worked significantly beyond this general reduction, nor in general, did they significantly increase the proportion of jobs that were part-time.[19]

3. Pre-UI and Post-UI Occupation

The final characteristic of the pre- and post-UI jobs that we examined is occupation. For treatments 1 and 3, we had no expectations about the impact of the treatments on occupation, but we did expect that treatment 2, because of the training, might lead to somewhat more occupational changes than would occur in the control group.

Data on the pre-UI and post-UI occupations of workers who found a post-UI job, and who reported both their pre-UI and post-UI occupation, are reported in Table VI.13 at the one-

---

[18] At least one of the treatments--the reemployment bonus--addressed this possibility by paying the bonus only to workers who found a full-time job.

[19] The JSA-only-control group difference in the probability of a part-time job is statistically significant at the 95 percent confidence level for a one-tail test, but the magnitude of the difference is small.

TABLE VI.12

PRE- AND POST-UI HOURS OF EMPLOYMENT PER WEEK

(Standard Errors in Parentheses)

	Treatment Group			
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group
Pre-UI Mean Hours Per Week	41.3 (0.29)	41.8 (0.23)	41.7 (0.30)	41.7 (0.23)
Post-UI Mean Hours Per Week	38.6 (0.36)	39.6 (0.29)	39.8 (0.32)	39.5 (0.27)
Difference of Means	-2.7	-2.2	-1.9	-2.2
Estimated Treatment Impact	-0.54 (0.42)	0.34 (0.37)	0.22 (0.41)	-- --
Percent Part-Time[a]	14.6	12.4	12.3	11.8
Sample Size	819	1,347	898	1,138

[a] A job was considered part-time if it was less than 32 hours per week. This definition was chosen because it was the definition used to determine if a job was eligible for the reemployment bonus.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE VI.13

PRE-UI AND POST-UI OCCUPATION CODES (ONE-DIGIT LEVEL) FOR RESPONDENTS WHO FOUND A JOB BEFORE THE INTERVIEW

SOC Occupation Code	Percentage of Sample Pre-UI	Percentage of Sample Post-UI	Difference between Pre-UI and Post-UI Percentage
1	14.1	11.5	-2.6
2	2.7	3.0	0.3
3	3.9	3.8	-0.1
4	24.1	28.4	4.3
5	6.6	8.3	1.7
6	10.5	10.7	0.2
7	27.0	21.6	-5.4
8	11.1	12.7	1.6

NOTE: Occupation was coded using the standard occupation codes (SOC) found in the Standard Occupation Classification Manual, 1980. Washington, D.C.: U.S. Department of Commerce, Office of Federal Statistical Policy and Standards, 1980.

Occupation Definitions

1 Executive, Administrative, and Managerial Occupations
Engineers, Surveyors, and Architects
Natural Scientists and Mathematicians
Social Scientists

2 Lawyers, Social Workers, and Religious Workers
Teachers, Librarians, and Counselors
Health Diagnosing and Treating Practitioners
Nurses

3 Pharmacists, Dieticians, Therapists, and Physician's Assistants
Writers, Artists, Entertainers, and Athletes
Technologists and Technicians

4 Marketing and Sales Occupations
Administrative Support Occupations, including Clerical

5 Service Occupations
Agricultural, Forestry, and Fishing Occupations

6 Mechanics and Repairers
Construction and Extractive Occupations
Precision Production Occupations

7 Production Working Occupations

8 Transportation and Material Moving Occupations

digit occupation level. These data show that the dominant pre-UI one-digit occupations among these workers were occupation 4 (sales, administrative support, and clerical) and occupation 7 (production-working occupations). Post-UI percentages for each occupation are shown in the second column of the table. The same two occupations, 4 and 7, were also dominant after UI receipt.

The difference in occupations in percentage terms across time indicate that, following UI receipt, occupation 4 became more dominant, while occupation 7 became less dominant. These results are not unexpected, since New Jersey has been experiencing an expansion of employment opportunities in the clerical/administrative support field and a decline in employment opportunities for production jobs.[20]

Another way to examine occupational change is to examine retention rates by occupational category (i.e., the percentage of workers in each occupation who were able to find jobs in the same occupation after being unemployed). Data on retention rates (Table VI.14) indicate that occupation 4 (sales, administrative support, and clerical) had the highest rate of retention (78 percent). Again, this is not surprising, given the employment growth experienced for these occupations. Somewhat more surprising is that the retention rate for occupation 1, which includes executive, administrative, and managerial occupations, as well as engineers and scientists, was the lowest (57 percent). These occupations are presumably also in demand in the state. One reason for this difference in retention rates could be that employers may not use observed unemployment spells as a signal of a worker's quality to the same extent in occupation 4 as they do in occupation 1. Thus, employers may have been more willing to hire individuals in occupation 4 who were laid-off by other employers than they were to hire similar individuals in occupation 1. Another reason could be that the incidence of temporary layoffs may differ between the two occupations.

Table VI.14 also provides the retention rates for each occupation by treatment group. It should be noted that for five of the eight occupations the retention rate was highest for treatment group 3. The higher retention rate for this group suggests that workers who were offered a reemployment bonus may have been more inclined to take a job in their own field than were workers in the other treatments. Because they may have wanted to find a job quickly in order to receive a bonus, they may have looked for a job in the occupation with which they were most familiar to facilitate their transition to reemployment.

---

[20] One industry in New Jersey which has been a large contributor to the loss of production jobs is the apparel industry. Production-job losses in the demonstration sites partly reflect this decline.

TABLE VI.14

RETENTION RATES FOR ONE-DIGIT OCCUPATIONS

		Treatment Group			
SOC Occupation Code	Full Sample	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group
1	56.6	52.3	58.5	56.1	58.1
2	67.8	62.1	68.5	79.0	62.2
3	60.4	52.5	56.7	70.8	59.0
4	77.7	78.5	77.6	79.1	75.9
5	63.4	42.4	66.7	76.8	67.0
6	70.5	67.9	70.7	76.8	66.9
7	62.6	62.6	64.6	64.3	69.8
8	68.1	69.3	68.3	68.9	66.4
Sample Size	4,202	819	1,347	898	1,138

NOTE: Occupation was coded using the standard occupation codes (SOC) found in the *Standard Occupation Classification Manual, 1980*. Washington, D.C.: U.S. Department of Commerce, Office of Federal Statistical Policy and Standards, 1980.

Occupation Definitions

1 Executive, Administrative, and Managerial Occupations
Engineers, Surveyors, and Architects
Natural Scientists and Mathematicians
Social Scientists

2 Lawyers, Social Workers, and Religious Workers
Teachers, Librarians, and Counselors
Health Diagnosing and Treating Practitioners
Nurses

3 Pharmacists, Dieticians, Therapists, and Physician's Assistants
Writers, Artists, Entertainers, and Athletes
Technologists and Technicians

4 Marketing and Sales Occupations
Administrative Support Occupations, including Clerical

5 Service Occupations
Agricultural, Forestry, and Fishing Occupations

6 Mechanics and Repairers
Construction and Extractive Occupations
Precision Production Occupations

7 Production Working Occupations

8 Transportation and Material Moving Occupations

To consider the issue of occupations further, we constructed binary variables to indicate when respondents switched occupations at the one-digit or two-digit levels. These variables (Table VI.15) show that the means for the one-digit and two-digit indicators were, respectively, 0.34 and 0.47 for the entire sample, indicating that 34 percent of the sample members switched occupations at the one-digit level, and 47 percent switched at the two-digit level. The treatment group means show that the highest rate of job-switching at the one-digit level occurred in treatment group 1, where 37 percent of the claimants changed occupation. The lowest rate occurred in treatment group 3, where only 31 percent of the workers switched.

The means for switching indicators at the two-digit occupational level reveal a similar pattern, with 48 percent of the treatment 1 members moving to a different 2-digit occupation, compared with 44 percent of treatment 3 members. However, except for the treatment 1/treatment 3 difference at the one-digit level, none of the differences among treatment groups or the treatment-control differences is statistically significant. Thus, while the data suggest that the bonus offer may have led to less occupational switching than occurred in treatment 1, the evidence on this point is relatively weak. Moreover, there is no indication that any of the treatments affected occupational switching relative to the control group.

4. Summary

In summary, our analysis of the characteristics of the post-UI jobs obtained by claimants shows that none of the treatments had adverse effects on hourly wage rates, hours worked, or occupations. In fact, there is evidence that each of the treatments had modest positive impacts on the hourly wages received by claimants. Thus, any concern that the treatments might have encouraged claimants to accept relatively poor jobs to become reemployed quickly is unfounded.

TABLE VI.15

MEANS OF INDICATORS FOR OCCUPATION-SWITCHING FOLLOWING UI RECEIPT

(Standard Error of Means in Parentheses)

		Treatment Groups			
	Full Sample	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group
Mean Indicator for Switch at One-Digit Level	0.34	0.37 (0.02)	0.33 (0.02)	0.31 (0.02)	0.33 (0.01)
Mean Indicator for Switch at Two-Digit Level	0.47	0.48 (0.02)	0.46 (0.01)	0.44 (0.02)	0.48 (0.02)
Sample Size	4,202	819	1,347	898	1,138

VII. BENEFIT-COST ANALYSIS

In this chapter, we combine estimates of the impacts of the NJUIRDP on earnings and UI benefits that were presented in earlier chapters with estimates of the net costs of the demonstration, so as to assess whether, compared with the existing UI system, the benefits of each treatment exceeded its costs, or vice versa. We examine benefits and costs from several perspectives--those of the major groups affected by these policies (claimants, employers, and the government budget) and the perspective of society as a whole. The purpose of this exercise is to summarize the information learned from the impact evaluation and cost estimation in a way that will help policymakers determine the relative desirability of implementing any of these treatments on an ongoing basis according to the relevant perspectives. Several issues are addressed in our benefit-cost evaluation:

- The costs of providing each of the three treatments on an ongoing basis relative to the costs of providing existing services (these are referred to as net costs)
- The effects of each treatment compared with existing services from the perspectives of society as a whole, claimants, employers, and the government (these are referred to as net effects)--that is, whether benefits outweigh costs or vice versa according to each perspective
- Whether the offer of training and relocation assistance in treatment 2 and/or the offer of the reemployment bonus in treatment 3 generated benefits which exceeded the additional costs of these two treatments once they went beyond the JSA services that comprised treatment 1
- How the costs and benefits of the treatments are allocated between "Department of Labor" programs (i.e., UI, ES, and JTPA programs at the local, state, and federal levels) and the rest of the government sector The implications of this distribution for funding future programs.
- The sensitivity of these conclusions to the assumptions underlying the benefit-cost analysis.

We explored these issues by using a comprehensive benefit-cost analytical framework developed by Mathematica Policy Research staff.[1] Section A discusses the components of this analytical methodology and the benefits and costs that are examined with it. (Appendix E discusses the technical aspects of the methodology and our assumptions in detail.) Section B presents our estimates of the net costs of the three treatments tested in the demonstration, and Section C presents the results of our benefit-cost analysis of the three treatments. Section D provides a summary and the conclusions of our analyses.

---

[1] See Long, Mallar, and Thornton (1981) for one of the initial applications of this framework.

A. METHODOLOGY

The comprehensive accounting framework used to compare the benefits and costs of the three treatments used in the NJUIRDP from the perspectives of society as a whole, UI claimants, employers, and the government encompasses the following steps:

- Defining the various perspectives whereby the benefits and costs to each of the relevant groups can be summed together to represent the benefits and costs to society as a whole
- Determining all potential costs and benefits from each perspective, including those whose value is more intangible and cannot be measured directly
- Assigning a dollar value to each benefit and cost
- Integrating all the benefits and costs from each perspective into a summary table to assess how much each element contributes to the "bottom line"
- Analyzing the sensitivity of the estimates to key assumptions to assess the effects of changing the assumptions on the overall benefit-cost analysis and its conclusions

1. The Various Perspectives

The benefit-cost analysis applied here encompasses a comprehensive accounting of all benefits and costs of the treatments from a number of perspectives. We consider the benefits and costs to the UI claimants themselves, to determine whether the treatments were beneficial to those whom they were designed to serve. We also consider the perspective of the employers who hired the claimants, to examine the net effects of their hiring decisions, and we consider the perspective of the government, to assess the budgetary impacts of each treatment relative to existing programs. We also break the government perspective down into "Labor Department" programs (that is, UI, ES, and JTPA) versus the rest of the network of government programs, to obtain more specific insight into the budgetary implications of these treatments.[2]

Together, these perspectives comprise all of society, and the benefits and costs to each perspective can be summed to derive the net benefits and costs to society as a whole. This social perspective measures whether each treatment increases or reduces the sum total of goods and services available in the economy. Economists refer to a change in goods and services overall as economic efficiency, because it indicates whether society as a whole is producing more with available resources. Transfers between the groups, such as taxes paid by employers or claimants, entail no net change in the resources available to society as a whole, and thus have no impact on the social perspective. Social benefits (or costs) are measured only as gains (or losses) for one group that are not balanced out by equal losses (or gains) for another group. However, transfers between groups have implications for the distribution of resources among groups, which is an important issue for any program.

---

[2] The Labor Department perspective and the rest of the government perspective combined encompass the federal, state, and local levels.

2. Listing Benefits and Costs from Each Perspective

After the relevant perspectives have been defined, the next step in the analysis is to construct a comprehensive list of the expected benefits and costs from each perspective, including those that are difficult to value in dollar terms--for instance, the psychological benefits to claimants of obtaining a job. Although such "intangible" benefits (or costs) are not susceptible to measurement, it is still important that they be assigned to the specific perspective(s), so that policy judgments can be made about how they are likely to affect the measured benefit-cost comparisons.

From the perspective of UI claimants, the key benefit of the demonstration treatments would be an increase in earnings and fringe benefits, due to more rapid reemployment (and/or higher earnings). More rapid reemployment should also be a psychological benefit to claimants, since most people find unemployment stressful. While the reemployment bonus and relocation assistance are direct transfers, the other services provided by the demonstration help claimants only if they increase their earnings. On the other hand, the increased reporting requirements under the demonstration imposed a cost on claimants by reducing their time for leisure or non-market work activities. Other costs to claimants are the loss in some UI benefits from more rapid reemployment, the additional taxes that they are required to pay on their increased earnings and any costs of working (e.g., child care or transportation). On the whole, we expect claimants to have derived net benefits from the demonstration.

Employers benefit from the value of the increased output produced by claimants who are hired more rapidly due to the treatment, but they also incur costs because they must compensate these employees through salaries and fringe benefits (which also include payroll taxes). We make the assumption that the value of the additional output to employers equals the value of the additional compensation by employers, which implies that they will incur no net benefits or costs from these treatments. However, this assumption may understate the benefits derived by employers from a better functioning labor market, which would reduce their recruiting and turnover costs. Alternatively, the treatments might impose a cost on some employers if some temporarily laid-off workers were unavailable for rehire. In addition, both employers and claimants could be affected by any changes in taxes due to an increase (or reduction) in government costs from offering the treatments. However, any such changes would occur only in the long run, and their effect would depend on how the treatments were funded. For this reason, they are not included in this analysis.

The Labor Department perspective includes the perspective of UI and ES, whose operations are funded through the UI payroll tax, and the perspective of the JTPA system, which is funded through general revenues. The costs of providing each of the three treatments in an ongoing program would be incurred by these three agencies. However, they would benefit from their direct share of the tax increases paid by claimants and their employers,[3] and from reductions in UI benefits paid to claimants and in the associated administrative costs of providing those benefits. In addition, their costs would be partially offset by a reduction in the costs of providing existing services. The question of whether the increase in payroll taxes to the Labor Department or the reduction in UI benefits was large enough to offset the net costs of the treatments is one of the key issues in this analysis.[4]

The other sectors of government inevitably derive net benefits from these treatments (assuming the existence of at least some positive impacts on the earnings of claimants) because they receive the portion of claimants' taxes that are not used directly to fund Labor Department programs.[5]

Again, all of these benefits and costs are summed across the perspectives to determine the benefits and costs to society as a whole. On the benefit side, the increased earnings of claimants are considered to represent an increase in total output and thus a net benefit to society. The assumption underlying this approach to valuing output is that the more rapid reemployment of demonstration claimants did not displace the employment of other individuals. This no-displacement assumption seems reasonable given the strength of the New Jersey economy. On the cost side, the net operational costs of offering each treatment represent social resources that could be spent otherwise, and are thus measured as net costs to society.

---

[3] In the analysis, we assigned an increase in UI payroll taxes to the Labor Department share, but we did not assume that the Labor Department would automatically obtain a share of any increase in other taxes.

[4] As noted on the previous page, the reduction in UI benefit receipt might, in the long-run, reduce UI tax rates, thus decreasing payroll taxes. This potential long-run effect is ignored in the analysis.

[5] Other governmental sectors could also benefit if the treatments reduced the receipt of such benefits as Food Stamps or other public assistance. This issue was examined in the analysis, but since no impacts were found we have not included these potential impacts in the benefit-cost framework.

3. Assigning Dollar Values to the Benefits and Costs from Each Perspective

The general approach used to value the benefits and costs of the three treatments is to measure the market value of the resources consumed, saved, or produced due to the treatment, as compared with the existing services available to UI claimants. The market value of these resources was estimated for the time period during which the demonstration operated, which corresponds roughly to fiscal year 1987.[e] Actual market prices were used whenever available, on the assumption that these prices are the best measure of the true costs of these resources. When market prices were not available, the dollar value of resources was estimated. For example, we estimated the value of fringe benefits, taxes, and the administrative costs of government agencies (see Appendix E for details on their estimation).

While it is reasonable to assume that the costs of the demonstration treatments occurred in the year after the initial claim, it is possible that the benefits are realized over a longer period of time. For treatments 1 and 3, the long-term benefits are expected to be small, because they focus on helping claimants become reemployed rather than on improving the level of the claimants' skills. A gain of a few more months of work experience is expected to have only a negligible effect on earnings after those months. However, for treatment 2, longer-term effects are considered to be much more plausible, since this treatment involved the offer of training. Those who took advantage of the training offer may ultimately earn higher wages than they would have earned otherwise, although they may work less in the short run. As discussed in the previous chapter, however, the available data provide no evidence that the impacts of treatment 2 would persist beyond a year after the initial claim filing date. We thus examined impacts on earnings only for the year after that date.

Our estimates of the net costs of providing the demonstration services are based on the estimates of demonstration costs that were prepared for the "Implementation and Process Report." In that report, we attempted to estimate the cost per claimant of offering each of the three treatments as an ongoing program. Costs that were plausibly due to the demonstration and limited-scale nature of the project were subtracted from estimates of the total expenditures of the demonstration.

Our focus in the benefit-cost analysis is a bit different--we wish to measure the net cost of each of the three treatments (again, using only those costs that would be incurred in an ongoing program) relative to the costs of the UI, ES, and JTPA services that are currently used by the target population, since claimants in the demonstration treatments received some services that they would have received in the absence of the demonstration. For example, some claimants who were referred by the demonstration to JTPA services would have gone to JTPA for services on their own. In order to measure the extent to which the costs of the demonstration services were greater than the costs of providing the existing services, we compared the costs of demonstration services with the costs of the services received by the control group.

---

[e] The exact time period over which each benefit and cost is measured is discussed in Appendix E. The differences in the time periods were sufficiently slight that adjustments for inflation were deemed unnecessary.

4. Comparing Benefits and Costs in a Summary Table

All of the data used in the benefit-cost analysis of each treatment are presented in summary tables, including figures for each benefit and cost from each perspective. This enables the reader to evaluate in one glance not just whether the treatment increased the goods and services available to society, but the extent to which it represents a redistribution of resources among groups. The role of an individual benefit or cost in determining whether the treatment is a net benefit or cost from a particular perspective can also easily be assessed--for example, an examination of the table reveals whether savings to the government budget generated by a treatment can be attributed to either low treatment costs or to large reductions in UI collected.

5. Sensitivity Analysis of Key Assumptions

The valuation of benefits and costs inevitably rests on a series of assumptions and a series of estimates which themselves are uncertain. Our goal is to lay out the underlying assumptions as clearly as possible so that it is possible for others to check for themselves the effects of changing the assumption. To that end, we also present some reasonable alternatives to the assumptions which we believe might be subject to change and may have substantial quantitative importance in the evaluation of the benefits and costs of these treatments, and we calculate the benefits and costs of the three treatments under these alternative assumptions. This approach also helps account for the uncertainty of the estimates by providing what we believe are reliable upper and lower bounds on the benefits and costs of each treatment.

B. ESTIMATES OF THE COSTS OF SERVICES TO TREATMENTS AND CONTROLS

In this section, we present our estimates of the per-claimant costs that would be incurred if each of the three demonstration treatments were offered as an ongoing program, and the reductions in the costs of existing ES, UI, and JTPA services that would occur. We then use these estimates to derive the net costs of the three demonstration treatments.

1. Demonstration Costs

Our estimates of per-claimant costs for each of the demonstration treatments are presented in Table VII.1. As designed, the JSA-only treatment (treatment 1) is the least expensive, at $169 per claimant, while the JSA plus training or relocation treatment (treatment 2) is the most expensive, at $491 per claimant; the JSA plus reemployment bonus treatment (treatment 3) falls in between, at $300 per claimant. The additional costs for treatments 2 and 3 are almost entirely due to the payments for training and for the reemployment bonus. In addition, the operational costs of treatment 2 are somewhat higher than the operational costs for the other treatments, since treatment 2 members received services from JTPA staff in addition to most of the same services that other treatment members received from ES and UI staff.

As one would expect, the largest proportion of operational costs are attributable to the cost of local office staff time, since these were the staff who provided services directly to claimants. The proportion of operational costs that represent local office staff time ranged from 68 percent of operational costs for treatment 3 to 76 percent for treatment 2.

The costs of payments to training vendors for treatment group 2 members more than equalled the operational costs of the treatment, since, although these services were used by relatively few treatment group 2 members, they involved substantial expenditures for each person in training.[7] The per-claimant cost of relocation assistance was negligible because only a handful of claimants used these services. The average reemployment bonus received per claimant in treatment 3 was $125, which is 42 percent of the total costs per claimant for treatment 3.

2. Costs of ES and UI Services to Controls

Our estimates of the gross costs of the treatments include the time devoted by UI and ES staff to providing services to claimants in the three treatment groups. Some of the same or similar services would have been provided to some treatment group members in the absence of the demonstration. Thus, it is necessary to estimate the costs that would have been incurred to provide these services to treatment members in the absence of the demonstration to estimate the net costs of the treatments.

For the UI system, gross treatment costs include the time devoted by the UI claims examiners and other UI staff to monitoring compliance with the demonstration's reporting requirements

---

[7] Our estimate of the average cost of classroom training provided by the demonstration is $2,723 per participant, and our estimate of the average cost of on-the-job training is $1,960 per participant (see Table IV.7).

TABLE VII.1

ESTIMATED GROSS COSTS PER CLAIMANT
FOR AN ONGOING PROGRAM

(Dollars)

	Treatment Group		
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus
Average Treatment Payments			
Classroom training	n.a.	224	n.a.
On-the-job training	n.a.	23	n.a.
Relocation assistance	n.a.	1	n.a.
Out-of-area job search	n.a.	1	n.a.
Transportation allowance	n.a.	1	n.a.
Bonus payments	n.a.	n.a.	125
Total	n.a.	250	125
Average Operational Costs			
Local office labor costs[a]	118	183	118
Central office labor costs[a]	25	29	30
Nonlabor costs plus administrative overhead[b]	26	29	26
Total	169	240	174
TOTAL PER CLAIMANT EXPENDITURES	$169	$491	$300

NOTE: Totals may not add due to rounding.

[a] Labor expenditures include all fringe benefits.

[b] This category includes direct items other than labor, as well as indirect costs.

n.a. = not applicable

and performing eligibility determinations if necessary. In order to estimate the net costs of this activity, we assumed that the cost of all nonseparation, nonmonetary determinations performed for treatment group members was included in demonstration-charged costs. We then estimated the cost of performing nonseparation eligibility determinations for control group members to obtain the net cost of this activity. This control group estimate was derived by using data on nonmonetary determinations presented in Table V.5 (there were an average of .415 determinations per control group member) together with a per-determination-cost figure supplied by NJDOL ($10.86). Thus, the cost of these determinations per control group member was roughly $5.

To obtain net ES costs, we assumed that all ES services provided to treatment group members were paid for by the demonstration. To calculate costs for controls, we used the data on the participation of control group members in ES services presented in Table IV.11, as well as administrative cost estimates for each service provided by NJDOL.[8] Overall, we estimated that Employment Service costs for providing services to the control group were $10 per person.

3. JTPA Service Use

Our approach for estimating the net costs for JTPA services was similar to the approach used for the ES services. We assumed that the JTPA training and operational costs attributed to the demonstration included all JTPA services to treatment 2 members.[9] We then estimated the costs of providing JTPA services to members of the control group and the other two treatment groups, since some JTPA services were received by all of these groups.[10] We estimated these costs based on the data on service use presented in Chapter IV and on cost data for JTPA costs per Title III enrollee.[11]

---

[8] Central ES office staff provided us with statewide estimates of the average amount of time involved in each service, and the appropriate salary rate for the staff providing the service. For services provided in groups, such as job-search workshops and testing, we used the average group size in the demonstration to compute a per-person cost for controls. These costs estimates were adjusted to account for the cost of supervision and non-personal service costs.

[9] Training for treatment 2 members that was funded by JTPA Titles IIa and III and the New Jersey Jobs Program was included in the figures for demonstration costs presented in Section B.1, along with the training funded directly by the demonstration.

[10] It was necessary to estimate JTPA costs for members of treatments 1 and 3 in addition to those for the control group, since the level of services received by these groups differed from the level received by the control group. This difference was due in part to the fact that the demonstration treatments substituted for some JTPA-provided services.

[11] The service-use data (Table IV.11) indicated that 4.8 percent of treatment 1 members, 4.6 percent of treatment 3 members, and 9.1 percent of the control group received some services from JTPA in addition to being certified as eligible. For the cost data, we used JTPA Title III average costs per enrollment in Program Year 1986 (July 1986 to June 1987) in each of the SDAs involved in the

Our estimates indicated that JTPA services cost about $99 per claimant in the control group and about $100 and $91 per claimant in treatments 1 and 3, respectively.

4. Net Costs of the Three Treatments Compared with Existing Services

In Table VII.2, we combine all of the cost data presented thus far in order to derive our estimates of the per-claimant net costs of each treatment relative to existing services. In keeping with the demonstration cost estimates presented in Table VII.1, our net cost estimates indicate that treatment 1 was least expensive (at $155 per claimant), treatment 2 was most expensive (at $377 per claimant), and treatment 3 was intermediate in cost (at $277 per claimant). These estimates of the net increases in the costs of providing each of the treatments in an ongoing program are smaller than the gross costs presented in Table VII.1. In particular, 8 percent of treatment 1 gross costs, 23 percent of treatment 2 gross costs, and 8 percent of treatment 3 gross costs were offset because some services were already provided to claimants.

C. BENEFITS AND COSTS FROM ALTERNATIVE PERSPECTIVES

In this section, we provide a set of benchmark estimates of the net benefits and costs of the three treatments relative to the existing services available to UI recipients. We also present the results of a number of sensitivity tests, which allow us to determine the extent to which our results will change if we change particular assumptions.

From the social perspective, we find that the benefits of treatments 1 and 3 substantially exceeded their costs when compared with existing services, while the benefits of treatment 2 only slightly exceeded its costs. All three treatments led to an increase in earnings, which provided net benefits to claimants. Treatments 1 and 3 created savings that exceeded costs from the perspective of the government sector as a whole, but both involved modest net costs to the Labor Department perspective. The rest of government benefited from the increased taxes paid by claimants, primarily on their increased earnings. In general, the results for treatments 1 and 3 were quite similar, while treatment 2 was expensive relative to the other treatments from all perspectives.

---

demonstration. Title III costs were used because the population eligible for Title III is more similar to those eligible for the demonstration than is the overall JTPA population. In using these cost data, we adjusted for the fact that individuals in treatments 1 and 3 who received JTPA services were more likely to receive training than were those in the control group.

TABLE VII.2

ESTIMATED NET COSTS PER CLAIMANT FOR AN ONGOING PROGRAM

(Dollars)

	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus
Demonstration Costs			
Classroom training	n.a.	224	n.a.
On-the-job training	n.a.	23	n.a.
Relocation assistance[a]	n.a.	3	n.a.
Reemployment bonuses	n.a.	n.a.	125
Local office labor costs[b]	118	183	118
Central office labor costs[b]	25	29	30
Nonlabor costs plus administrative overhead[c]	26	29	26
Total	169	491	300
Nondemonstration JTPA Costs	100	n.a.	91
Offsetting Costs of Existing Services			
ES costs	-10	-10	-10
JTPA costs	-99	-99	-99
UI costs	-5	-5	-5
Total	-114	-114	-114
Net Costs per Claimant	155	377	277

NOTE: Totals may not add due to rounding.

[a] Relocation assistance includes out-of-area job search and transportation allowances.

[b] Labor expenditures include all fringe benefits,

[c] This category includes direct items other than labor, as well as indirect costs.

Our sensitivity tests are intended to show the range of benefit-cost estimates that arise from using alternative assumptions that may be better-suited to some policymakers. The estimated net benefits of all three treatments from the perspective of claimants would be substantially smaller but still positive if we relied on the wage-records data to derive earnings impact estimates. This change would yield net costs to government while the costs from the Labor Department perspective would be roughly the same. Our cost estimates, we believe, are conservative, but we show that a number of seemingly large changes in our assumptions about costs reduce the total costs of the treatments by only modest amounts.

1. Benefit-Cost Analysis of Treatment 1 versus Existing Services

Table VII.3 presents benchmark estimates of the net benefits (or costs) of treatment 1 (intensive job-search assistance) relative to existing services. In general, treatment 1 led to a substantial increase in output and earnings and a moderate reduction in UI benefits collected. As designed, the costs of the treatment were relatively low. We estimate that the benefits of the treatment outweighed the costs from the perspectives of claimants, the government, and society as a whole. The net gain to the government sector, however, was derived from a small net loss to the Labor Department sector and a larger gain to the rest of the government. That is, the reduction in UI payments was insufficient to pay for the cost of the increased services.

More specifically, we estimate that members of treatment 1 increased their earnings by an average of $608 relative to members of the control group, and we impute another $128 in additional fringe benefits to reach a total increase of $736 in compensation. Much of this increase in compensation benefited the claimants, but treatment costs were modest enough, and enough of the increased earnings were returned to the government sector via increased taxes and reduced UI benefits that the government sector realized a net gain of $88 per claimant.[12] When we specifically examine the UI, ES, and JTPA programs which comprise the Labor Department sector, however, we find that they incurred a (modest) net loss of $61 per claimant.

Society as a whole gained from the increased output and reduced expenditures on government services. The net social gain, which can be thought of as an indicator of the efficiency of the treatment, is estimated to be $581 per claimant.

---

[12] There were no observed impacts of the treatment on participation in income support programs other than UI which is why no benefits or costs are reported for these programs. For the UI program there was a reduction in benefits paid and a corresponding small reduction ($1) in the administrative costs of processing claims.

TABLE VII.3

BENEFIT-COST COMPARISON OF THE JSA-ONLY TREATMENT WITH EXISTING SERVICES

(Dollars per Claimant)

				Government		
Benefits and Costs	Society	Employers	Claimants	Labor Dept.	Other Government	Government Total
Market Output and Wages						
Increased output	736	736	0	0	0	0
Wages and fringe benefits	0	-736	736	0	0	0
Tax Payments						
Claimants' taxes	0	0	-156	7	149	156
Income Support Payments						
UI payments	0	0	-87	87	0	87
Other programs	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI payment administration	1	0	0	1	0	1
Administration of other programs	0	0	0	0	0	0
Demonstration Costs						
Classroom training costs	0	0	0	0	0	0
OJT costs	0	0	0	0	0	0
Relocation assistance	0	0	0	0	0	0
Reemployment bonuses	0	0	0	0	0	0
Local office labor costs	-118	0	0	-118	0	-118
Central office labor costs	-25	0	0	-25	0	-25
Other costs (direct and indirect)	-26	0	0	-26	0	-26
Offsetting Costs of Existing Services						
ES costs	10	0	0	10	0	10
JTPA costs	-1	0	0	-1	0	-1
UI costs	5	0	0	5	0	5
Sum of Measured Benefits and Costs	581	0	493	-61	149	88
Nonmonetary Factors						
Psychological benefits of earlier reemployment	+		+			
Burden of reporting requirements, reduced leisure time, costs of work	-		-			

NOTE: Totals may not add due to rounding.

2. Benefit-Cost Analysis of Treatment 2 versus Existing Services

The benefit-cost comparison between treatment 2 and existing services is presented in Table VII.4. Overall, treatment 2 provided net benefits to claimants and net costs to the government sector, while society as a whole roughly broke even.

Our estimates suggest that the earnings of treatment 2 members were an average of $345 higher than those of control group members, and we impute an additional $72 of increased fringe benefits. These increased earnings and fringe benefits represent a benefit to claimants on average, which was partially offset by an increase in taxes of $81 per person, and a reduction in UI benefits also of $81 per person. The estimated net benefit per claimant of treatment 2 (compared with existing programs) was $258.

The very substantial costs of providing training, even to the minority of treatment 2 members who pursued this option, meant that the government sector incurred net costs for treatment 2. Treatment 2 members were three to four times as likely to pursue training as were control group members. In addition, they received the same set of initial job-search services from the ES as did those in treatment 1. The increased taxes and reduced UI benefits generated by the increased employment of persons in treatment 2 substantially offset the costs of the JSA services, but did not begin to cover the costs of the training itself. The net government loss of $214 per claimant can be broken down into a $291 loss for Labor Department programs, and a $78 gain for the rest of the government.

From the perspective of society as a whole, the choice between treatment 2 and existing services appears fairly even, with a slight net benefit of $44 per person in favor of the treatment.

Compared with treatment 1, treatment 2 benefits were lower relative to costs from all perspectives. In particular, treatment 2 members realized lower earnings increases on average, but the costs of the treatment were much higher. It seems that the most plausible interpretation is that treatment 2 members who entered training (or hoped to enter training) deferred reentering the labor market, and did not increase their earnings capacity sufficiently to compensate for the fewer number of weeks that they worked in the year after the claim filing date. The possibility that the impacts of treatment 2 on earnings are underestimated because we use only the year after the claim filing date is explored below.

TABLE VII.4

BENEFIT-COST COMPARISON OF THE JSA TRAINING OR RELOCATION ASSISTANCE TREATMENT WITH EXISTING SERVICES

(Dollars per Claimant)

				Government		
Benefits and Costs	Society	Employers	Claimants	Labor Dept.	Other Government	Government Total
Market Output and Wages						
Increased output	417	417	0	0	0	0
Wages and fringe benefits	0	-417	417	0	0	0
Tax Payments						
Claimants' taxes	0	0	-81	4	78	81
Income Support Payments						
UI payments	0	0	-81	81	0	81
Other programs	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI payment administration	1	0	0	1	0	1
Administration of other programs	0	0	0	0	0	0
Demonstration Costs						
Classroom training costs	-224	0	0	-224	0	-224
OJT costs	-23	0	0	-23	0	-23
Relocation assistance	-3	0	0	-3	0	-3
Reemployment bonuses	0	0	0	0	0	0
Local office labor costs	-183	0	0	-183	0	-183
Central office labor costs	-29	0	0	-29	-	-29
Other costs (direct and indirect)	-29	0	0	-29	0	-29
Offsetting Costs of Existing Services						
ES costs	10	0	0	10	0	10
JTPA costs	99	0	0	99	0	99
UI costs	5	0	0	5	0	5
Sum of Measured Benefits and Costs	44	0	258	-291	78	-214
Nonmonetary Factors						
Psychological benefits of earlier reemployment	+		+			
Burden of reporting requirements, reduced leisure time, costs of work	-		-			

NOTE: Totals may not add due to rounding.

3. Benefit-Cost Analysis of Treatment 3 versus Existing Services

The results of our benefit-cost analysis of treatment 3--job search assistance plus the reemployment bonus--are presented in Table VII.5. Treatment 3 members experienced similar earnings gains to claimants in treatment 1 and larger UI benefit reductions than did the other two treatment groups, while the costs of the treatment fell in between the costs of treatments 1 and 2. On balance, there was a substantial net gain to society of $565 per claimant relative to existing services.

Claimants experienced a net benefit of $510 on average, comprising a $591 increase in earnings and a $124 increase in fringe benefits, balanced by a $170 reduction in UI benefits and a $161 increase in taxes. The government benefited overall, but Labor Department programs suffered a net loss of $99 per claimant, because the added cost of the bonus payments was larger than the savings in benefits and the additional taxes paid. The rest of the government sector experienced a net gain of $154 from an increase in taxes.

Overall, the findings for treatment 3 were quite similar to those for treatment 1. The earnings gains experienced by claimants were similar, and while the bonus payments represented a cost to the government sector and a gain to claimants, this cost (and gain) was partially offset by the larger reduction in UI payments relative to treatment 1. However, because this reduction in UI payments plus the increase in taxes relative to treatment 1 did not completely offset the cost of the bonus, this treatment, as implemented, was $33 more expensive per claimant for the government sector than was treatment 1.

4. The Sensitivity of the Estimates to Alternative Assumptions

The estimates presented in the preceding sections are based on a number of assumptions which could be questioned. The estimates of UI and earnings impacts that are used are also subject to some uncertainty. Thus, it is useful to consider the sensitivity of the estimates to changes in the assumptions or estimates that are used to generate the measured benefits and costs. We first consider the implications for the analysis had we decided to use earnings impact estimates based on the wage-records data rather than estimates based on the interview data. The implications of the possibility that the impacts of treatment 2 may persist beyond the time period that is observed are also explored.

TABLE VII.5

BENEFIT-COST COMPARISON OF THE JSA PLUS REEMPLOYMENT BONUS TREATMENT WITH EXISTING SERVICES

(Dollars per Claimant)

				Government		
Benefits and Costs	Society	Employers	Claimants	Labor Dept.	Other Government	Government Total
Market Output and Wages						
Increased output	715	715	0	0	0	0
Wages and fringe benefits	0	-715	715	0	0	0
Tax Payments						
Claimants' taxes	0	0	-161	7	154	161
Income Support Payments						
UI payments	0	0	-170	170	0	170
Other programs	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI payment administration	1	0	0	1	0	1
Administration of other programs	0	0	0	0	0	0
Demonstration Costs						
Classroom training costs	0	0	0	0	0	0
OJT costs	0	0	0	0	0	0
Relocation assistance	0	0	0	0	0	0
Reemployment bonuses	0	0	125	-125	0	-125
Local office labor costs	-118	0	0	-118	0	-118
Central office labor costs	-30	0	0	-30	0	-30
Other costs (direct and indirect)	-26	0	0	-26	0	-26
Offsetting Costs of Existing Services						
ES costs	10	0	0	10	0	10
JTPA costs	8	0	0	8	0	8
UI costs	5	0	0	5	0	5
Sum of Measured Benefits and Costs	565	0	510	-99	154	55
Nonmonetary Factors						
Psychological benefits of earlier reemployment	+		+			
Burden of reporting requirements, reduced leisure time, costs of work	-		-			

NOTE: Totals may not add due to rounding.

A second set of sensitivity tests explore the possibility that we have overstated the costs of providing the demonstration treatments as ongoing programs. We believe that the benchmark estimates presented above represent upper-bound estimates of the costs of providing the three treatments, since these estimates do not take into account the fact that the demonstration may have operated less efficiently than would be the case in an ongoing program. Thus, we present some alternative cost estimates which are based on fairly arbitrary assumptions about the degree to which efficiency gains might be realized. For this reason, these alternative estimates should be viewed as a set of lower-bound estimates. However, it should be noted that while costs might be lower in an ongoing program relative to the actual costs in NJUIRDP a reduction in costs and staff effort might also affect the outcomes of the program. Thus, these lower-bound estimates should be viewed with caution.

a. Alternative Estimates of Earnings Impacts

We chose to use what we believe are the best estimates of the impacts of the demonstration as our estimates of the benefits of each treatment. In particular, we have used estimates of earnings impacts drawn from the follow-up interview data. The wage-records data provide an alternative set of estimates of earnings impacts, which in all cases are lower than those estimated from the interview data. As discussed in Chapter VI, the wage-records data understate earnings impacts because (1) the data do not include the earnings of the self-employed and those who find new employment outside New Jersey, and (2) the data cover the first calendar quarter after the claim date, and thus miss treatment-control differences in the first month or two after the claim filing date for much of the sample. Other results suggest that this is the point at which much of the impacts of the treatments occur.

Our first sensitivity tests calculated the benefits and costs of the three treatments using the estimates of earnings impacts from the wage-records data. The results from these tests are presented in Tables VII.6-VII.8. For all three treatments, using earnings estimates based on the wage records greatly reduces the estimated benefits to claimants, government, and society as a whole relative to our benchmark estimates. The net benefits and costs from the Labor Department perspective scarcely change, because we are not changing the impacts of the treatment on UI benefits. For treatments 1 and 3, however, the net benefits to claimants and society as a whole remain positive, while remaining more expensive to the government. If the wage-records data were an accurate measure of earning impacts for treatment 2, the treatment would on balance be expensive from all perspectives, even for claimants (since, as before, their UI benefits decline).

TABLE VII.6

SUM OF BENEFITS AND COSTS OF THE JSA-ONLY TREATMENT UNDER ALTERNATIVE ASSUMPTIONS

(Dollars Per Claimant)

			Government		
	Society	Claimants	Labor Dept.	Other Government	Government Total
Benchmark Estimates	581	493	-61	149	88
Using Earnings Impacts from Wage-Records Data	41	83	-66	25	-41
With a 50 Percent Reduction in Central Office Labor Costs	594	493	-48	149	101
With Adjustment for Scale	587	493	-55	149	94
With a 50 Percent Reduction in Central Office Labor Costs and Adjustment for Scale	600	493	-42	149	107

NOTE: Totals may not add due to rounding.

TABLE VII.7

SUM OF BENEFITS AND COSTS OF THE JSA PLUS TRAINING OR RELOCATION ASSISTANCE TREATMENT UNDER ALTERNATIVE ASSUMPTIONS

(Dollars Per Claimant)

			Government		
	Society	Claimants	Labor Dept.	Other Government	Government Total
Benchmark Estimates	44	258	-291	78	-214
Using Earnings Impacts from Wage-Records Data	-286	7	-295	2	-293
With a 50 Percent Reduction in Central Office Labor Costs	59	258	-277	78	-199
With a 50 Percent Reduction in Central Office and JTPA Local Office Costs	91	258	-245	78	-167
With Adjustment for Scale	52	258	-284	78	-206
Using Demonstration Measure of JTPA Participation	91	258	-245	78	-167
With a 50 Percent Reduction in JTPA and Central Office Costs, Adjustment for Scale, and Demonstration Measure of JTPA Participation	145	258	-191	78	-113

NOTE: Totals may not add due to rounding.

TABLE VII.8

SUM OF BENEFITS AND COSTS OF THE JSA PLUS REEMPLOYMENT BONUS TREATMENT UNDER ALTERNATIVE ASSUMPTIONS

(Dollars Per Claimant)

			Government		
	Society	Claimants	Labor Dept.	Other Government	Government Total
Benchmark Estimates	565	510	-99	154	55
Using Earnings Impacts from Wage-Records Data	240	263	-102	79	-23
With a 50 Percent Reduction in Central Office Labor Costs	580	510	-84	154	70
With Adjustment for Scale	571	510	-92	154	62
With a 50 Percent Reduction in Central Office Labor Costs and Adjustment for Scale	586	510	-77	154	77

NOTE: Totals may not add due to rounding.

Another area of possible uncertainty is the persistence of earnings impacts into the future. Based on the interview sample for whom we have fifth- and sixth-quarter earnings, we do not find significant earnings impacts after the first year for any of the treatment groups. As noted earlier, this finding makes sense for treatments 1 and 3, but is more surprising for persons in treatment 2, since some of them participated in training for much of the year after the initial UI claim filing date, and thus, although we would expect any earnings impacts for treatment 2 members to appear later than those for the other treatment members, we would also expect them to persist for a longer period of time. If treatment 2 earnings impacts persisted for three years (at the level measured with the interview data), the treatment would generate net benefits that would exceed those of existing services from the perspectives of claimants and society as a whole. However, this level of persistence would not be enough for the government sector to realize benefits relative to existing services.

b. Alternative Estimates of Net Costs

In our estimates of treatment costs, we have attempted to be conservative in our assumptions, so that when benefits exceed costs under these assumptions we can be fairly confident that benefits do indeed exceed costs. Our cost estimates should thus be considered as an upper bound on true costs.

One reason for believing that the true costs of an ongoing treatment may be lower than our benchmark estimates is that staff costs might be lower in an ongoing program, even though we excluded costs for staff whose functions were clearly associated with the research and planning aspects of the demonstration. Staff costs may be overstated for three major reasons: (1) less central office staff time may be required for effecting coordination among divisions in an ongoing program, (2) JTPA staff may devote less time, since the proportion of those offered training who expressed interest was less than anticipated, and (3) an ongoing program might operate at a more efficient scale, since the demonstration used roughly the same number of staff in each office, even though several served smaller caseloads. In order to examine these issues, we repeat here three sensitivity analyses discussed in more detail in the "Implementation and Process Report," and consider their results for the benefit-cost analysis, rather than the analysis of costs alone. These three tests are to (1) recalculate costs under the assumption that central office labor costs fall by 50 percent, (2) recalculate costs under the assumption that JTPA local office costs fall by 50 percent, and central office staff costs fall by 50 percent, and (3) recalculate local office labor costs using data only from the 6 sites which served the largest numbers of treatment-group members, to adjust for the fact that some sites operated at a smaller scale than did others, with the same level of staff. These three changes both by themselves and together provide us with plausible lower bounds for the operational costs of the treatments.

In addition, we may have overestimated training costs. A number of treatment 2 members are recorded as having received classroom training or OJT in the JTPA data system, but not in the records maintained by the JTPA counselors who were working on the demonstration. Thus, we do not know the costs of their training. We assumed that their training cost was the same, on average, as the training of the group of training participants for which we have cost data. Investigations by NJDOL staff, however, suggest that the training received by these members of treatment 2 may have been less expensive, either because they did not complete the program or because they were referred to a less expensive type of training program than were other trainees (sometimes funded under JTPA Title IIA, rather than under Title III). Rather than attempting to track down training expenditures for these cases, we simply examined training expenditures solely for those training participants who were tracked by the demonstration, a figure which we know provides a lower-bound estimate of expenditures on training.

The results of these tests are also presented in Tables VII.6 through VII.8. In general, they lead to slight reductions in costs (or increases in benefits) for the Labor Department sector, the government as a whole, and society. The two tests that applied to treatment 2 alone--the 50 percent reduction in JTPA costs and central office costs--and using the lower measure of participation in training reduced estimated costs by the most substantial amounts ($46, or about 10 percent of demonstration costs, for each change). Moreover, the combination of these changes with the other cost changes would have increased the net social gain for treatment 2 from $44 to $145. However, this increase in net social gain does not change our overall conclusions about our comparisons of the three treatments.

D. SUMMARY AND DISCUSSION

The results of our benefit-cost analysis suggest that all three treatments offer net benefits to society as a whole and to claimants when compared with existing services. Treatments 1 and 3 also lead to net gains to the government sector as a whole, but not to the Labor Department agencies which actually offered the services. Treatment 2 is expensive for the government sector.

When we compare treatment 2 with treatment 1, we find that treatment 2 costs were higher (or benefits were lower) than those for treatment 1 from all viewpoints, since the costs of the service component of treatment 2 were higher and its earnings gains were substantially lower. Treatment 2 would look better if earnings gains could be sustained for at least three years, but the limited data we have on earnings in the fifth and sixth quarters after the claim filing date do not show any earnings differences between treatment 2 members and control group members.

The benefits and costs of treatment 3 appear to be similar to those of treatment 1 from all perspectives, although it generated slightly higher costs from the government perspective. This occurred because the greater reductions in UI benefits in treatment 3 relative to treatment 1 did not fully balance the cost of the bonus from the government perspective.

Our sensitivity analyses showed that our decision to rely on the interview data for measures of earnings impacts rather than on the wage- records data was important--but that treatments 1 and 3 would have positive net benefits from the social perspective even with the wage-records data. In addition, these analyses showed that the differences between our upper-, and lower-bound estimates of treatment costs do not lead to major changes in the "bottom-line" estimates of the net benefits (or costs) of the three treatments.

These findings suggest that it would not be possible for any of the treatments to be funded solely from the savings in UI benefits and increased UI taxes (although treatment 1 comes closest). Treatments 1 and 3 could be funded from the reduction in UI benefits and the increase in tax revenues overall, if it were possible to reallocate tax money that would currently go to other sectors of the government to cover the additional costs of these programs. Treatment 2 would need to be funded either through a reduction in spending on other programs or an increase in taxes, since it appears to create net costs to the government as a whole.

VIII. IMPLICATIONS OF THE FINDINGS FROM THE DEMONSTRATION EVALUATION

The findings presented in the previous chapters indicate that the demonstration was generally successful at identifying and offering services to a group of claimants who, in the absence of the demonstration, would have experienced difficulties in becoming reemployed. The demonstration treatments were also successful at providing services to these individuals early in their unemployment spells and at increasing their level of service use relative to what would have been the case with the existing service system. Each of the treatments also appears to have hastened the reemployment of claimants, and thus, both earnings rose and the amount of UI benefits collected declined relative to the levels observed for claimants who were not offered the treatments. Finally, relative to existing services, each of the treatments appears to have generated net benefits to society as a whole and to claimants. Two of the treatments were also beneficial from the standpoint of the government as a whole, although none of them generated net benefits from the narrower perspective of the Labor Department agencies that operated the demonstration.

These overall, positive findings suggest that the demonstration treatments represent potentially useful reemployment policies that could be directed toward UI claimants. However, several issues associated with the treatments bear further discussion: (1) the targeting of demonstration services, (2) the use of participation requirements, and (3) the selection of services.

A. TARGETING SERVICES

An important issue for any reemployment strategy is deciding who should be provided with services. The eligibility definition used in the New Jersey demonstration attempted to target services toward displaced workers who would experience reemployment difficulties. Our evaluation indicated that, although some individuals who were made eligible for services faced good reemployment prospects and some who were not eligible faced poor reemployment prospects, the particular eligibility definition chosen for the demonstration was generally successful at achieving its target objective. The reemployment experience of members of the demonstration-eligible control group were, on average, considerably worse than those of individuals who were not eligible for the demonstration.

However, our evaluation also showed that, among demonstration- eligibles, the treatments, particularly the job-search-assistance-only treatment, were most successful at promoting the reemployment of individuals who had marketable skills, such as clerical and other white-collar workers.[1] The treatments were less successful for individuals who faced hard-core, structural unemployment problems, such as blue-collar workers, workers from durable-goods manufacturing industries, and permanently separated workers. That is, the displaced workers with more severe reemployment problems may have been affected less by the treatments than were other workers who faced relatively more favorable reemployment prospects.

This finding is perhaps not surprising, since each of the treatments emphasized self-directed job search by providing an intensive set of job-search services (including a job-search workshop) during the initial service period. These services are designed for individuals who have marketable skills, and it appears that this group was the one that found jobs more readily than they would have otherwise.

This finding also suggests that the treatments, particularly the initial mandatory job-search assistance services, are appropriate and cost-effective for a broad-range of UI claimants who meet reasonable operational definitions of displacement, but that longer-run, more intensive services are needed for displaced individuals who face major structural dislocations. The demonstration did offer occupational training in the second treatment (see further below), but additional services may be needed. For example, the high rate of excusals from testing and the job-search workshop for language and literacy reasons suggests that referrals to English as a Second Language or remedial education services may be needed for some individuals.

B. USING PARTICIPATION REQUIREMENTS

The job-search assistance components of the treatments instituted in New Jersey included a UI requirement that claimants report for services. Failure to report for services could have led to the denial of benefits. Our analysis of this requirement and of the compliance process associated with it found evidence that this requirement was enforced reasonably well. Most individuals who did not report for the initial job-search services were subject to a benefit eligibility review or had some clear reason for nonreporting (e.g., they stopped claiming benefits, or they moved from a demonstration area to another area). Moreover, the rate at which the eligibility of claimants was questioned and denied was greater for members of the treatment groups than for members of the control group.

---

[1] A New Jersey Department of Labor demonstration of job-search assistance run prior to the NJUIRDP had success with a similar group of claimants. That demonstration focused intensive job-search assistance on claimants whose occupations were considered to be in demand, and the evaluation found that this treatment led to a substantial reduction in the duration of UI benefits (2.6 weeks) and in total benefits paid ($340) over the benefit year (NJDOL, 1987).

Given these findings it is important to ask if a program in which participation in the services was voluntary would have produced the same results. The design of the demonstration does not enable us to address this question directly, since all of the treatments encompassed the same structure of mandatory components. However, some indirect evidence is available that the mandatory nature of the treatments was important. Participation levels in the initial services were higher than one would expect in a voluntary program. Some of the largest impacts observed for subgroups occurred for individuals who expected to be recalled, a group that would not have been likely to respond to a voluntary program. And much of the impact of the demonstration appeared to occur very quickly. For example, the weekly exit rate from UI was higher for treatment members than for control group members beginning with the fifth week of unemployment when claimants were initially called-in for services. We conclude that the reporting and compliance processes were probably important contributing factors to the impacts of the treatments.

C. SELECTING SERVICES

The demonstration tested three packages of services: (1) job-search assistance (JSA) only, (2) job-search assistance plus training or relocation assistance, and (3) job-search assistance plus a reemployment bonus. Several observations can be made about the relative effectiveness of these three treatments.

Our findings generally found that the job-search assistance component of the treatments was successful at promoting the reemployment of claimants. Our examination of impacts on UI receipt showed that the JSA-only treatment reduced the amount of UI received. It also showed that the rate of exit from the UI system increased early in individuals' claim spells, a time period in which intensive job-search assistance was provided. Similarly, we found positive impacts on employment and earnings early in individuals' claim spells. The benefit-cost analysis also indicated that the JSA-only treatment generated a net social benefit. Our findings indicated that the addition of the training or relocation offer to the basic job-search assistance services did not lead to larger short-run impacts. In fact, as could be expected, the impacts were slightly smaller, because individuals who entered training continued to collect UI and delayed their return to employment. Moreover, since the cost of the training itself is high, the training treatment was expensive relative to the other treatments, even though only a small percentage of individuals received training. However, these findings should not be viewed as indicating that training should not be offered. Training could have longer-run impacts which we were not able to measure in this study, and it may potentially be valuable for the individuals who do not have marketable skills on whom the treatments had little impact.

Our findings about the reemployment bonus offer showed that the amount of UI benefits received by claimants who were offered the bonus was significantly less than the amount received by claimants who were not. Differences in employment and earnings between those who were offered the bonus and those who were not were positive for the first two quarters after the claim filing date although these differences did not persist into later quarters. Only the first quarter earnings impact was statistically significant. These findings concerning the impact of the bonus offer on UI receipt and earnings suggest that the bonus offer helped hasten the reemployment of claimants. However, our benefit-cost analysis indicated that the additional UI savings generated by the bonus offer did not offset the cost of the bonus itself, and that the gains in earnings were not sufficiently greater than those obtained from the JSA-only treatment that they make a difference in the benefit-cost comparisons. Overall, the job-search assistance-only and job-search assistance plus reemployment bonus treatments had very similar benefit-cost outcomes from all perspectives. Thus, the New Jersey demonstration evaluation results suggest that a reemployment bonus offer does not improve labor-market outcomes sufficiently to make the combination of mandatory job-search assistance plus the bonus offer a more successful treatment than mandatory job-search assistance alone. An unanswered question is how a bonus offer by itself would compare with job-search assistance alone, particularly if job-search assistance contains mandatory elements, as was the case in New Jersey. Two other demonstrations, in Pennsylvania and Washington, are exploring a wide-range of reemployment bonus schemes.

REFERENCES

Bloom, H.S. and J. Kulik. "Evaluation of the Worker Adjustment Demonstration: Final Report." Cambridge, MA: Abt Associates, July 1986.

Congressional Budget Office. "Promoting Employment and Maintaining Income with Unemployment Insurance." Washington. D.C.: Congressional Budget Office, March 1985.

Corson, W. and S. Dunstan. "Implementation and Process Report." Part 2 in The New Jersey Unemployment Insurance Reemployment Demonstration Project: Final Evaluation Report. Princeton, NJ: Mathematica Policy Research, 1989.

Corson, W., S. Kerachsky, and E. Eliason-Kisker. "Work Search Among Unemployment Insurance Claimants: An Investigation of Some Effects of State Rules and Enforcement." UI Occasional Paper 88-1. Washington, D.C.: U.S. Department of Labor, Employment and Training Administration, 1988.

Corson, W., D. Long, and W. Nicholson. "Evaluation of the Charleston Claimant Placement and Work Test Demonstration." UI Occasional Paper 85-2. Washington, D.C.: U.S. Department of Labor, Employment and Training Administration, 1985.

Corson, W., D. Long, and R. Maynard. "An Impact Evaluation of the Buffalo Dislocated Worker Demonstration Project." Princeton, NJ: Mathematica Policy Research, March 1985.

Corson, W. and W. Nicholson. "An Analysis of UI Recipients' Unemployment Spells." UI Occasional Paper 83-1. Washington, D.C.: U.S. Department of Labor, Employment and Training Administration, 1985.

Corson, W. et al. "The Design of the New Jersey Unemployment Insurance Reemployment Demonstration Project." Princeton, NJ: Mathematica Policy Research, 1986.

Crosslin, R., J. Hanna, and D. Stevens. "Using Unemployment Insurance Data to Predict Economic Hardship from Displacement." In Displaced Workers: Implications for Educational and Training Institutions, edited by K. Hollenbeck, F.C. Pratzner, and H. Rosen. Columbus, OH: The National Center for Research in Vocational Education, The Ohio State University, 1984.

Flaim, P.O. and E. Sehgal. "Displaced Workers of 1979-83: How Well Have They Fared?" Monthly Labor Review, vol. 108, no. 6, June 1985, pp. 3-16.

Gordon, N. "Job Displacement and Employment Security: Reactor Comments." In Displaced Workers: Implications for Educational and Training Institutions, edited by K. Hollenbeck, F.C. Pratzner, and H. Rosen. Columbus, OH: The National Center for Research in Vocational Education, The Ohio State University, 1984.

Gustman, A. "Analyzing the Relationship of Unemployment Insurance to Unemployment." Working Paper no. 512. Cambridge, MA: National Bureau of Economic Research, July 1980.

Hamermesh, D. Jobless Pay and the Economy. Baltimore, MD: Johns Hopkins University Press, 1977.

Kalbfleisch, J. and R. Prentice. The Statistical Analysis of Failure Time Data. New York, NY: Wiley Press, 1980.

Kiefer, N. "Economic Duration Data and Hazard Functions." Journal of Economic Literature, vol. 24, no. 2, June 1988, pp. 646-679.

Kulik, J., D.A. Smith, and E. Stromsdorfer. "The Downriver Community Conference Economic Readjustment Program: Final Evaluation Report." Cambridge, MA: Abt Associates, Inc., May 18, 1984.

Long, D., C. Mallar, and C. Thornton. "Evaluating the Benefits and Costs of the Job Corps." Journal of Policy Analysis and Management, vol. 1, no. 1, 1981, pp. 55-76.

N.J. Department of Labor. "Evaluation of the Perceivable Demand List: Pilot Project." Trenton, N.J.: N.J. Department of Labor, Division of Planning and Research, Office of Program Research, October 1987.

Richardson, P. et al. "Referral of Long-Term Unemployment Insurance Claimants to Reemployment Services." Washington, D.C.: Macro Systems, Inc., July 1988.

U.S. Department of Labor. "Alternative Uses of Unemployment Insurance." UI Occasional Paper 86-1. Washington, D.C.: U.S. Department of Labor, Employment and Training Administration, 1986.

Vroman, W. "Innovative Developments in Unemployment Insurance." Washington, D.C.: National Commission for Employment Policy, Research Report Series, RR-85-02, February 1985.

Westat. "Job Search and Relocation Assistance: Final Report." Rockville, MD: Westat, 1981.

Woodbury, S.A. and R.G. Spiegelman. "Bonuses to Workers and Employers to Reduce Unemployment: Randomized Trials in Illinois." The American Economic Review, vol. 77, no. 4, September 1987, pp. 513-530.

APPENDIX A
DATA SOURCES

This appendix briefly describes the six main data sources used for the impact analysis of the New Jersey UI Reemployment Demonstration Project. These are: (1) the Participant Tracking System, (2) UI administrative records, (3) quarterly wage records, (4) Employment Service administrative records, (5) JTPA administrative records, and (6) the follow-up interview. Data from the first five sources are available for all claimants in the sample while the interview data are available for a subsample (see Chapter II for a description of the two samples).

A. THE PARTICIPANT TRACKING SYSTEM

A Participant Tracking System (PTS) was developed for the sample selection and assignment task and for use in the provision of the demonstration treatments. This system included (1) summary data from the UI data system, (2) data on claimant characteristics collected on a short questionnaire administered to all claimants during their Benefit Rights Interview, (3) data from the sample selection process, and (4) data on claimant's participation in the demonstration services. All four types of data were used in the analysis.

The summary data from the UI system included data on claimants' age, race, and sex together with data on base period earnings, the initial claim, the UI entitlement, nonmonetary determinations, and the amount of UI collected. The claimant questionnaire included variables used for eligibility determination such as recall status, tenure on the pre-UI job, and use of a union hiring hall. The date of selection into the sample and the treatment-control assignment came from the sample selection process.

Finally, the majority of the PTS data items provided information on claimants' participation in the demonstration including scheduled dates for services and the services received by claimants. Data for both the initial job-search assistance services and subsequent services such as training, relocation assistance and the reemployment bonus were included.

B. UI ADMINISTRATIVE RECORDS

In addition to the UI data which were added to the PTS and which are described above, UI records for each weekly claim were collected. These records included the date of claim, the date of any payment, the amount of payment, any deductions for earnings, pensions, or other reasons, and an indicator explaining the disposition of the claim (paid or reason for non-payment). These records were used to compute the duration of the first UI spell and to examine the importance of some of the eligibility screens.

C. WAGE RECORDS

Quarterly wage records were collected for each individual in the sample for quarters 1986.3 through 1988.1. Individuals with multiple employers in some quarters have multiple wage records for those quarters. Each wage record includes the year and quarter, an employer identifier, the amount of wages, and the number of weeks worked in the quarter. These records were used to construct variables for total wages and weeks worked per quarter following an individual's date of claim and a variable indicating if an individual returned to his/her pre-UI employer.

D. EMPLOYMENT SERVICE DATA

Data on ES registration, characteristics of registrants, and ES service receipt were collected from the Employment Service data base. These data were used to construct variables describing the services received from the ES.

E. JTPA DATA

Data on JTPA certification and JTPA service receipt were collected from the JTPA data base. These data were used to construct a set of variables describing the services received through JTPA.

F. FOLLOW-UP INTERVIEW DATA

A follow-up interview was administered to a subset of claimants (see Appendix B for a description). This interview included data on the pre-UI job, job-search activities, experience with the demonstration treatments, post-UI employment, spouse employment, other post-layoff income, demographic characteristics not available from administrative records, and work history. These variables were used in the analysis directly and to construct summary variables describing, primarily, post-UI employment and earnings.

APPENDIX B

SURVEY RESULTS AND NONRESPONSE ANALYSIS

The survey data used in this report were collected during the period from January 6 through August 3, 1988. The survey was a mixed-mode data collection, in which sample members who could not be contacted by telephone were contacted, if possible, in person. This appendix presents the results of this survey and examines potential nonresponse bias that could arise with use of these data.

A. SURVEY RESULTS

This section summarizes the results of the survey and the outcomes of the telephone and field efforts, and discusses the major reasons for nonresponse.

1. Overall Results

The results of the survey are presented in Tables B.1 and B.2. Of the 8,195 sample members on whom interviews were attempted, 5,828 responded to an interview, yielding an overall response rate of 71 percent. Not surprisingly, the propensity of sample members to respond to an interview paralleled their degree of participation in and receipt of services from the program: only 62 percent of those who had been assigned to a treatment group but had not participated in any program activities responded; this was the lowest response rate of any sample group, including noneligibles. The response rate rose to 68 percent for those treatment group members who had attended the orientation session but went no further in the program, it rose further to 74 percent for those whose final program activity was the assessment and counseling interview, and it was highest (82 percent) for those who received training or relocation assistance or a reemployment bonus from the program. The response rate for the control group sample was roughly equivalent to that for the overall treatment sample: of the 2,055 control group sample members for whom interviews were attempted, 1,469, or about 72 percent, responded. As expected, the noneligible sample group's response rate trailed behind that obtained for the treatment and control group samples; only 468 of the 743 noneligible sample members for whom interviews were attempted responded, for a response rate of 63 percent.

2. Telephone Interviewing Results

As mentioned above, an initial attempt was made to interview sample members by telephone, and of the 5,828 interviews that were completed, 91 percent (Table B.2) were administered from the telephone center. The overall response rate achieved by the telephone center was 65 percent (5,285 completed interviews out of the total release of 8,195), indicating that the overall response rate would have been reduced by about 6 percentage points (at the maximum), if sample members who could not be contacted by telephone had not been pursued in the field.

TABLE B.1

RESULTS OF SURVEY BY TREATMENT CONTROL STATUS

	Treatments													
	Did not Attend Orientation		Attended Orientation But Not Assessment		Attended Assessment		Added Services		Existing Services		Noneligibles		Total	
	No.	Percent	No.	Percent	No.	Percent	No.	Percent	No.	Percent	No.	Percent	No.	Percent
Interview Completed	520	62.1	483	67.8	2,434	73.9	454	82.3	1,469	71.5	468	63.0	5,828	71.1
Interview Refused[a]	54	6.4	40	5.6	163	5.0	12	2.2	106	5.2	39	5.3	414	5.1
Sample Member Not Contacted/Not Located[b]	212	25.3	147	20.7	586	17.8	80	14.5	395	19.2	198	26.6	1,618	19.7
Sample Member Claimed Did Not File for UI	10	0.1	5	0.7	6	0.2	0	0.0	12	0.6	7	0.9	40	0.5
Other Outcome Statuses[c]	42	5.0	37	5.2	106	3.2	6	1.1	73	3.6	31	4.2	295	3.6
Total Sample Released	838	100.0	712	100.0	3,295	100.0	552	100.0	2,055	100.0	743	100.0	8,195	100.0

[a] Includes partially completed interviews that were terminated because of a refusal to continue, proxy refusals at the sample members' request, and cases in which access to the sample member was denied.

[b] Includes sample members who could not be located and who could be located but not contacted, and sample assignments that were discontinued because the data collection period came to an end.

[c] Includes deceased and institutionalized sample members, sample members that were unavailable during the data collection period, and sample members who could not respond to an interview due to language barriers or physical or mental handicaps.

TABLE B.2

FINAL STATUS BY INTERVIEWING MODE

	Central Telephone Facility	Field Interviewer Telephone	In-Person	Final Statuses	
				No.	Percent
Interview Completed	5,285	176	367	5,828	71.1
Interview Refused	360	4	50	414	5.1
Sample Member Not Contacted/ Not Located	1,050	10	558	1,618	19.7
Sample Member Claimed Did Not File for UI	39	0	1	40	0.5
Other Outcome Statuses	258	2	35	295	3.6
Total Sample Resolution	6,992	192	1,011	8,195	100.0

3. Field Interviewing Results

Sample members were generally assigned for in-person interviewing when they could not be contacted by telephone.[1] Altogether, 15 percent of the total sample that was released was resolved in the field. Of the 1,203 field assignments, 543, or 45 percent, resulted in completed interviews, representing 9 percent of the 5,828 completions.

4. Nonresponse

As depicted in Tables B.1 and B.2, the most prevalent reason for interview nonresponse was an inability to locate or to contact sample members during the data collection period; altogether, 1,618 sample points, or 20 percent of the total sample release fell into this category. Of the 1,618, the locations of 470 were known with a reasonable degree of certainty, but they could not be contacted because: (1) they either did not live within a reasonable proximity of New Jersey or they lived in locations remote from field interviewers and had no telephone or an unpublished telephone number; (2) they were not at home during repeated visits by field interviewers; (3) they lived in limited-access security buildings; (4) their last known address was a post office box and they did not respond to letters requesting contact; or (5) they did not respond to a message or did not appear for an appointment. Of the remaining 1,148 sample points in the uncontactable/unlocatable category, no trace of 575 was discovered during the data collection period, and 361 of the 1,618 sample points, had moved subsequent to their UI claims, and, as a result, could not be located. Finally, 212 of the 1,618 sample points were located, but their assignments were discontinued because the data collection period came to an end.

The noneligible sample group proved to be the most mobile and difficult to locate and contact; 27 percent of this group were not found or not contacted during the data collection period. Somewhat surprisingly, control group sample members were less difficult to locate and contact than were treatment group members who had little or no involvement with the program. As expected, sample members who received training or relocation assistance or the reemployment bonus were the least difficult to locate and contact: only 15 percent could not be approached for an interview for these reasons. Finally, the uncontactable/unlocatable rate for treatment group members whose last program activity was the assessment and counseling interview roughly corresponded with that for the control group: 18 percent of this group could not be contacted or located during the data collection period.

---

[1] Exceptions to this rule were made when the telephone search led to assignment of a final status, such as deceased, in which an interview could not be completed. In addition field follow-ups were not made when the respondent was believed to live in a remote location where interviewing would have been prohibitively expensive.

The second most common reason for interview nonresponse was refusals; 414 sample points, or 5 percent of the total sample release, fell into this category. Refusals came in three ways: (1) termination of an interview in the early stages and refusal to continue upon being recontacted; (2) refusal to start an interview, repeatedly broken appointments or otherwise demonstrated evasive behavior, and (3) the sample member's household refused at the sample member's request or otherwise denied access to the sample member. Not surprisingly, the highest refusal rates were incurred among treatment group members who either had not participated in the program or had dropped out of the program in the early stages. In refusing an interview, many sample members in these groups stated that they felt the program had been an intrusion upon their private lives, that the program had imposed or had threatened to impose an unsought, unrequested, and unnecessary burden upon them, and that being asked to respond to an interview was an extension of a disagreeable process. The noneligible and control group refusal rates were similar to each other: 5 percent of each group. This refusal rate was roughly replicated by treatment group sample members who had received no further program services after the assessment and counseling interview. Many of the individuals in this sample group who refused an interview also cited the intrusiveness and burden of the program, and more than a few were distressed because they had not been selected for or had not received a desired service. Finally, as expected, treatment group sample members who had received training or relocation assistance or the reemployment bonus proved to be the most cooperative of all sample members; only 12 out of the release of 552, or 2 percent, refused to participate in the survey.

The remaining 335 sample points, or slightly less than 4 percent of the total sample release, were resolved in a variety of ways. Forty claimed that they had neither filed a claim for nor collected UI during the relevant period; 39 were deceased; 21 were incarcerated or institutionalized; 19 could not respond to an interview due to physical or mental incapacitation; 28 were unavailable for the duration of the data collection period, and 188 could not respond to an interview in either English or Spanish.

B. POTENTIAL NONRESPONSE BIAS

Nonresponse bias could potentially arise with use of the interview data because approximately 29 percent of the individuals with whom interviews were attempted did not respond to the interview. As noted above, this nonresponse occurred primarily because some potential respondents could not be located. Some potential respondents also refused to be interviewed.

To analyze the implications of this nonresponse for the analysis of survey data we used administrative data that are available for both respondents and nonrespondents. These data include both demographic and other baseline data that are used as control variables in the analysis and outcome data for UI benefits and earnings (from wage records data). Thus, unlike many such analyses, we can directly examine the implications of nonresponse for impact estimation.

Before proceeding with that analysis, one methodological issue needs to be addressed. This issue arises because the survey sample was stratified to obtain a higher proportion of interviews from individuals who participated in demonstration services than the proportion than occurred in the entire demonstration sample. Because of this design, weights were constructed for the survey respondent sample for use in the impact analysis. Each cell in the design was given a weight such that the weighted distribution within each treatment group matched that of the overall sample. In order to compare the survey respondents and nonrespondents, the nonrespondent group was also assigned weights. Again the weights were assigned by cell. These weights were assigned so that the resulting weighted distribution by treatment group was the same for nonrespondents as for respondents.

Turning to our analysis of nonresponse, we first examine differences between respondents and nonrespondents for selected baseline characteristics. These differences, which are reported in Table B.3, show that the respondents were older, and also more likely to be female and white than nonrespondents. On average, they also had higher wages in the base period, and, as a result, they had higher average weekly benefit amounts for UI. Finally, the nonrespondents were more likely to expect recall than the respondents. Many of these differences are statistically significant within the treatment and control groups. In general, these findings indicate that the respondent sample displayed the demographic characteristics of a more stable population than those of the nonrespondent sample. This is not surprising given that the main reason for survey noncompletion was a failure to locate the claimant. The higher nonresponse rate for the "other race" category arises also because of the difficulties that occurred in attempting to interview Hispanics who did not speak English.[2]

Data on UI receipt and earnings following the UI claim date (Table B.4) also show some differences between respondents and nonrespondents. For example, the proportion exhausting UI

---

[2] The other race category includes Hispanics, Asians, and Native Americans.

TABLE B.3

CHARACTERISTICS OF INTERVIEW SAMPLE
BY TREATMENT GROUP AND
SURVEY RESPONSE STATUS
(Weighted Means)

	Treatment Group							
	JSA Only		JSA Plus Training/ Relocation		JSA Plus Reemployment Bonus		Control Group	
	Responders	Nonresponders	Responders	Nonresponders	Responders	Nonresponders	Responders	Nonresponders
Demographic Variables								
Percent female	51.3	39.6	50.4	39.6	48.1	39.4	50.3	41.8
Percent black	15.0	22.7	17.4	25.0	15.4	24.2	13.2	25.5
Percent other race	16.6	26.7	16.8	30.6	17.0	30.7	18.1	31.2
Percent age < 35 years	26.8	34.1	27.5	34.1	28.9	35.4	28.3	33.1
Percent age $\geq$ 55 years	25.0	17.0	23.6	19.1	23.6	18.6	24.5	18.4
Base Period Employment								
Mean earnings (dollars)	19,034	16,259	18,804	16,872	19,461	15,317	19,013	15,864
Percent in durable manufacturing	18.1	18.6	18.1	18.6	19.2	18.2	19.1	18.3
Percent in nondurable manufacturing	17.3	19.7	17.9	21.2	17.2	19.8	18.2	23.0
Percent expecting recall	33.0	40.1	33.1	42.7	33.0	42.9	32.9	41.0
UI Entitlement								
Mean weekly benefit rate (dollars)	182.3	174.4	181.9	178.6	186.3	172.8	185.1	175.0
Mean potential weeks duration	25.0	24.7	25.1	24.6	25.2	24.6	25.0	24.6
Sample Size	1,047	375	1,692	602	1,152	415	1,469	540

TABLE B.4

UI AND EARNINGS OUTCOMES
BY TREATMENT GROUP AND
SURVEY RESPONSE STATUS
(Weighted Means)

	Treatment Group							
			JSA Plus Training/ Relocation		JSA Plus Reemployment Bonus			
	JSA Only						Control Group	
	Responders	Nonresponders	Responders	Nonresponders	Responders	Nonresponders	Responders	Nonresponders
UI Receipt								
Mean dollars paid in benefit year	3,147	3,129	3,133	3,105	3,160	3,020	3,273	3,195
Mean weeks paid in benefit year	17.4	18.0	17.3	17.7	17.0	17.4	17.8	18.4
Mean weeks paid in first spell	15.0	15.5	15.0	15.1	14.9	14.6	15.4	16.1
Exhaustion Rate	41.4	43.9	41.3	45.0	40.8	44.6	44.3	48.8
Earnings[a] (dollars)								
Quarter 1	1,903	1,268	1,775	1,426	1,931	1,342	1,831	1,213
Quarter 2	2,355	1,851	2,330	1,790	2,452	1,846	2,312	1,784
Quarter 3	2,717	2,187	2,709	2,238	2,740	2,166	2,664	2,083
Quarter 4	2,559	2,039	2,749	2,204	2,690	2,165	2,697	2,137
Sample Size	1,047	375	1,692	602	1,152	415	1,469	540

[a] Quarters are calendar quarters following the date of claim.

benefits and the number of weeks paid was slightly lower for the respondents than nonrespondents, while the total amount of benefits received was slightly higher due to the higher average weekly benefit. Similarly earnings tended to be higher for respondents than nonrespondents which could be expected given the differences in baseline characteristics. However, unlike the demographic differences, the differences in these outcome variables are not statistically significant.

In conclusion, the survey nonrespondents display some significantly different demographic characteristics than respondents, but these differences do not appear to affect systematically any particular treatment group or the controls. Also, the important UI and earnings analysis variables showed no significant differences between the respondent and nonrespondent groups. Ultimately, then, analysis of treatment impacts undertaken using the survey data should be free of problems due to nonresponse.

APPENDIX C

COMPARISON OF INTERVIEW AND WAGE RECORDS DATA

Both administrative (wage records) and interview data on employment and earnings were collected and used in the analysis of employment and earnings impacts that was presented in Chapter VI of the impact and benefit-cost report. As reported in that chapter, the estimates obtained from the two data sources differed with the interview-based impact estimates exceeding those obtained from the wage records data. Although both sets of impact estimates were presented and both were used in the benefit-cost analysis, we argued that the interview-based estimates were likely to be more reliable for the NJUIRDP evaluation than the wage records estimates. This argument was based primarily on the fact that, because the wage records data applied to calender quarters, it was not possible to use the wage records data to focus on impacts that occurred immediately following the date of claim. This was an important problem for the analysis since the analysis of UI impacts suggested that most of the impact of the demonstration occurred soon after the date of claim.

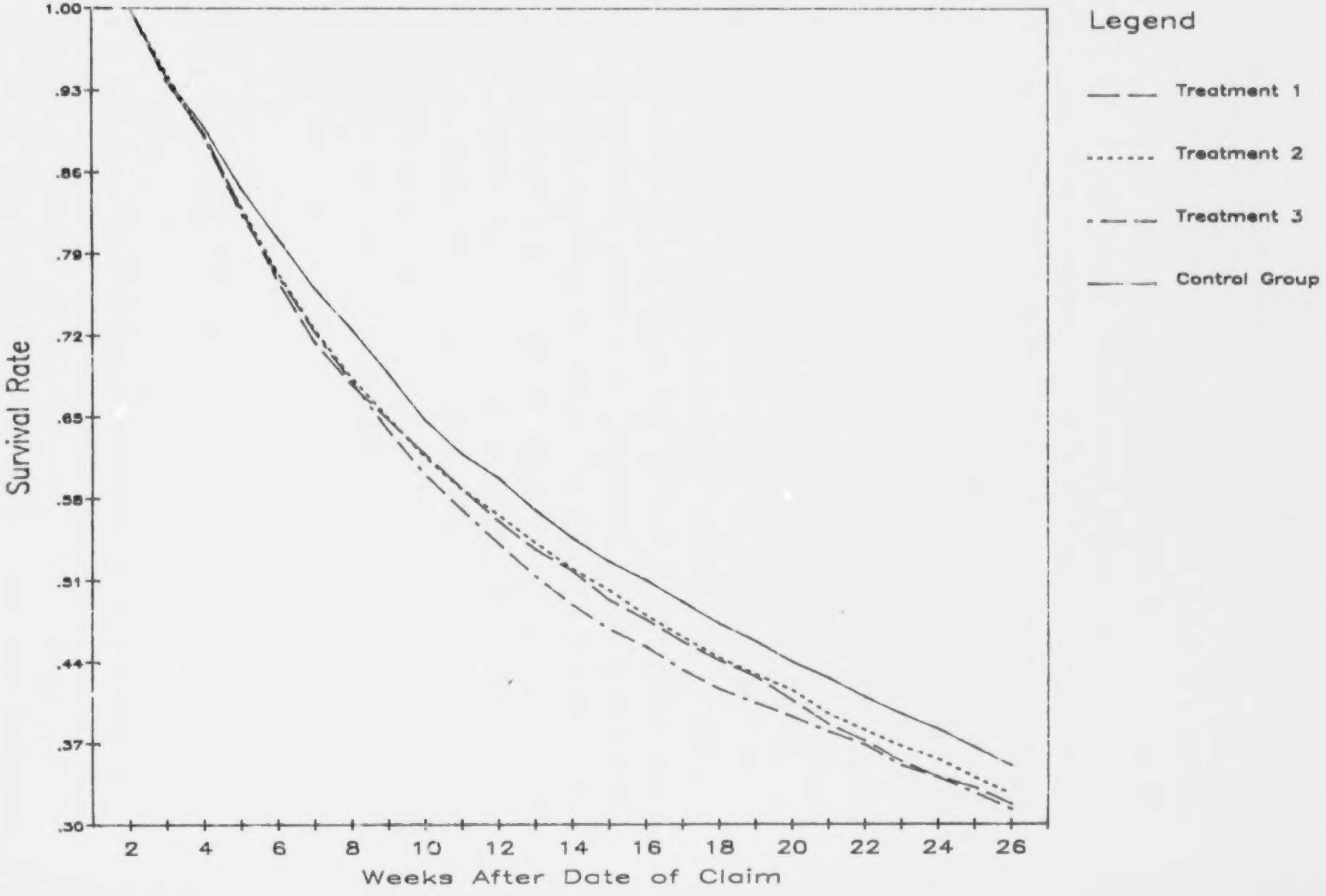
In this appendix we provide a comparison of the wage records and interview data on earnings to provide some insight into whether our decision to highlight the interview-based estimates was sensible. We make this comparison since measurement error is generally expected to be more of a problem for interview data than wage records data.[1] Non-response is also generally expected to be more of a problem for interview data than wage records data although our analysis of interview non-response in Appendix B suggests that non-response was not a major problem for analysis using this survey. Non-response could also be a potential problem for analysis using the wage records data since the earnings of any claimants employed outside the state of New Jersey or employed outside the UI-covered sector (e.g.,,he self-employed) are not captured by NJ wage records. If such individuals are included in any earnings impact analysis with zero earnings, impact estimates will be biased downward.

To examine the importance of the potential shortcomings of both data sets we examined earnings data for the fourth quarter of 1987. We chose the fourth quarter of 1987 for analysis because it was the latest quarter for which we had complete wage records data for every claimant in the

---

[1] Interview data are subject to measurement error because of faulty recall by respondents concerning employment dates or wage rates of jobs. Wage records data can also be inaccurate because of misreporting of wages or social security numbers. Conversations with NJ state staff suggest that such misreporting may affect about 5 percent of the wage records. Wage records data can also present a problem for analysis since earnings are reported when they are received as opposed to when they are earned. For example, some individuals in the sample appear to have received, after their date of claim, sizeable lump sum amounts of severance pay from their pre-UI employers. These payments could be misinterpreted as representing earnings from a post-UI job.

FIGURE V.1
NUMBER OF WEEKS IN FIRST UI SPELL - SURVIVAL RATES



An examination of Figure V.1 reveals some interesting differences among the survival rates over time. For the first eight weeks after the claim filing date, the survival rates for the treatment groups were nearly equal. Since the treatments were identical during the first 8 weeks (on average, assessment/counseling interview was held during the eighth week after the claim filing date), this result is expected. However, the plots for this initial time period also show that the difference between the treatment and control groups began to show up at about week five, which was the point at which orientation occurred, and the difference grew rapidly over weeks 5 to 8. This pattern suggests that the treatments (JSA at this point) had an immediate impact on the claimants who were selected for the demonstration treatments.

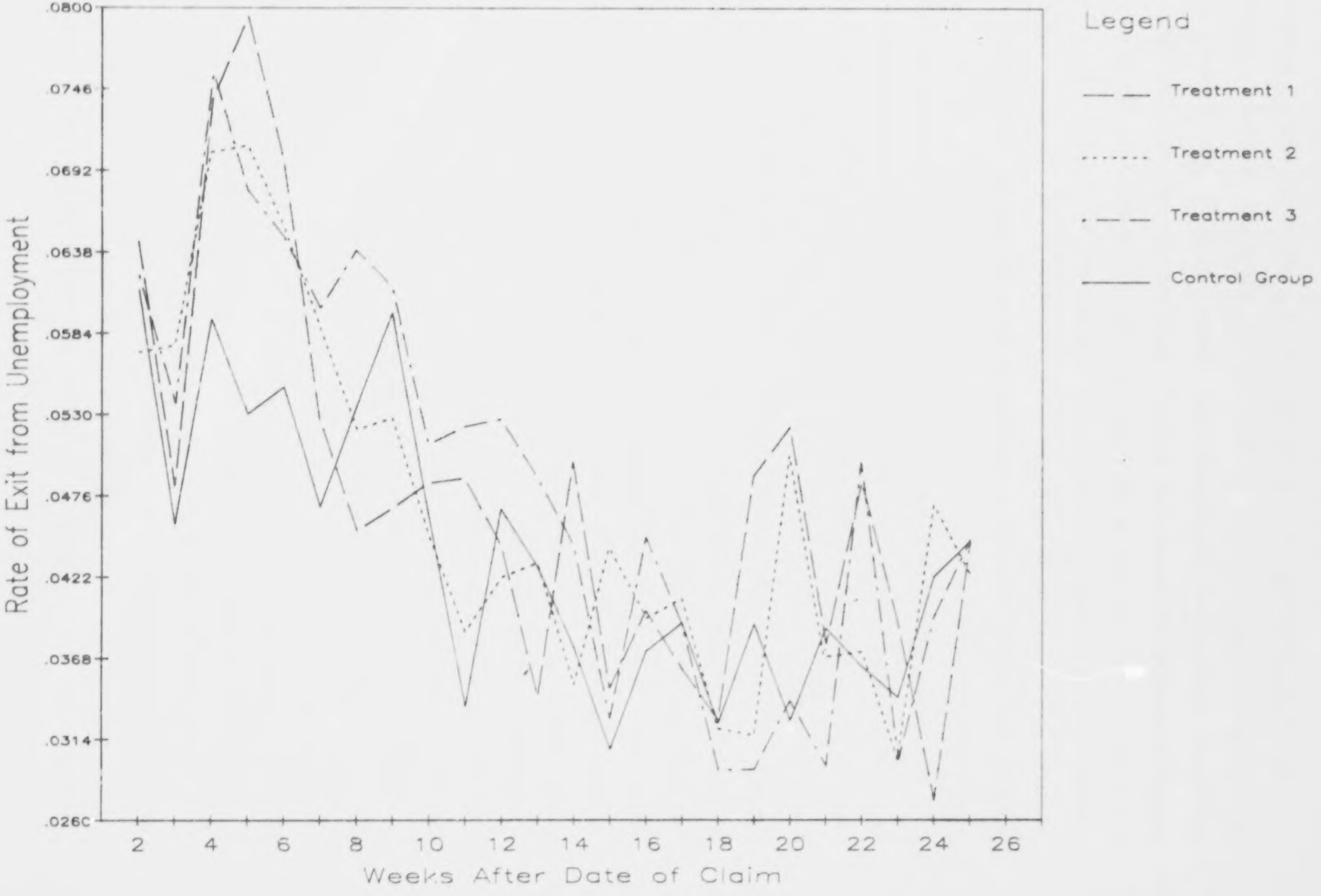
Beginning at about week 9, the survival rate for treatment 3 was noticeably less than the rates for the other treatment groups. This difference reflects the impact of the reemployment bonus offer, which provided an additional incentive to become reemployed quickly. The survival rates for treatments 1 and 3 converged around week 24. This convergence of the survival rates suggests that the reemployment bonus probably had a minor impact on claimants who would have exhausted benefits in the absence of the offer. Instead, the impact appears to have been to shorten the spells of claimants who would not have exhausted their benefit entitlements. For treatment 2, the survival rate plot was almost identical to that of treatment 1, except for the tail, where it appears that slightly more individuals in this treatment exhausted benefits. This result is probably due to the fact that individuals who entered training continued to collect benefits during their training period. A final point to note is that the survival rates of all the treatment groups remained below those of the control group at week 26, suggesting that the treatments had an impact on the duration of unemployment beyond the 26th week following the claim filing date.

Figure V.2 presents the plotted exit rates for each of the treatment and control groups. Because exit rates are volatile over time, it is difficult to draw treatment-control comparisons, but the exit rates do clearly show the effect of the demonstration treatments during the first 8 weeks after the claim filing date. That is, the exit rates of the treatment group exceeded those of the control group. The overall pattern of the rates is also interesting. The exit rates of all of the treatment and control groups were highest during the first 10 weeks following the claim filing date. After that point, the rates declined gradually, followed by a small increase approaching the point of exhaustion.

In summary, this analysis of survival rates and exit rates for UI claimants clearly supports the conclusion that the demonstration treatments reduced the weeks of UI payments received by

best copy available

FIGURE V.2
NUMBER OF WEEKS IN FIRST UI SPELL - EXIT RATES



claimants, with treatment 3 members showing the largest impact on weeks paid. The probability of remaining on UI was noticeably lower for all three treatment groups after the 5th week following the claim filing date, and the difference still existed at 26 weeks after that date.

D. IMPACTS OF THE TREATMENTS ON NONMONETARY DETERMINATIONS AND DENIALS

Each of the three treatments tested in the NJUIRDP required that claimants report to the demonstration office for orientation. Claimants were also expected to report for the remaining initial services--testing, the job-search workshop, and assessment/counseling--if they were scheduled to receive these services. Finally, following assessment, claimants were expected to report periodically to the demonstration office if they did not enter training or return to work.

Compliance with these reporting requirements was also monitored. Attendance was recorded by ES staff in the participant tracking system, which generated a weekly "delinquency report" that listed all claimants who failed to attend one of the initial scheduled events. This report was taken to the local UI office, and a claims examiner in each office was expected to "pend" the automated UI files for all individuals listed on the delinquency report. If these individuals claimed additional UI benefits, the pend indicator would not permit a payment to be made, and a fact- finding interview was triggered. The fact-finding interview and, if appropriate, a determination of eligibility were performed following NJ UI laws and regulations. Whether eligibility was denied depended on the reasons given during the fact-finding interview for failure to report for demonstration services, but claims examiners were instructed to inform claimants that failure to report without good cause could lead to benefit denial until the claimants reported and were rescheduled for services.

The procedures for monitoring claimants' compliance with the periodic reporting requirements following assessment were not as rigorous and systematic as those for the initial services. ES staff were expected to report any noncompliance to UI on an individual basis, but, as discussed in the "Implementation and Process Report," few instances of noncompliance were recorded in the tracking system.

These reporting requirements and the compliance process were expected to increase both the probability that a determination of eligibility would be made for nonseparation reasons and the probability that a denial would occur. As can be seen in Table V.6, increases did indeed occur, and the differences between each treatment group and the control group are statistically significant. On average, a nonmonetary determination was performed for 40 percent of the treatment group

TABLE V.6

NONSEPARATION ISSUE, NONMONETARY DETERMINATIONS, AND DENIALS

(Percent)

	Treatment Group			
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group
Number of Determinations				
0	59.1	59.2	62.1	72.0
1	25.8	26.5	24.6	18.9
2	9.4	9.3	7.9	6.0
3	4.3	3.5	3.6	2.2
4	1.4	1.6	1.8	1.0
Number of Denials				
0	80.5	81.5	82.4	85.5
1	14.4	14.0	13.2	11.1
2	4.2	3.3	3.1	2.3
3	0.6	0.9	1.1	0.7
4	0.3	0.3	0.2	0.3
Percent of Determinations Leading to a Denial	47.7	45.3	46.4	51.8
Sample Size	2,416	3,810	2,449	2,385

NOTE: The analysis is restricted to the first four nonmonetary determinations for any claimant during the current benefit year.

members, compared with 28 percent of the control group members; benefit denials were made for 19 percent of the treatment group members and 15 percent of the control group members. However, as these numbers imply, the probability that a determination led to a denial was lower for treatment members than for control group members (46 versus 52 percent). This finding suggests that the additional determinations that arose because of the demonstration reporting requirements yielded fewer denials than did a typical nonmonetary determination.

Further insight into the nonmonetary determination process in the demonstration can be obtained from the data presented in Table V.7 on the issues that were adjudicated. These data clearly show that the primary difference in the determinations of treatment and control group members was that those for treatment group members were more likely to involve reporting requirement issues than were those for controls. About one-quarter of the issues adjudicated for treatments were reporting issues, compared with 13 percent for controls. The distribution of nonseparation issues among the other major categories was similar for treatments and controls.

Data in the remainder of the table clearly show why the additional determinations performed on treatment members relative to control group members did not increase the overall denial rate as much as the increase in the determination rate. First, it was less likely that more than one issue was considered for treatment members than for control group members, thus reducing the probability of a denial for the treatment groups. And, second, data on the probability that consideration of a issue led to a denial show no significant treatment-control differences, except for the reporting requirement issue, the one issue that occurred more frequently for treatment members than for control group members. However, the probability that noncompliance with this requirement led to a denial was less than 25 percent for treatment members, compared with 44 percent for control group members, a statistically significant difference.

These findings suggest that claims examiners were reluctant to disqualify claimants for failing to report for services, if noncompliance with this requirement was the only issue raised in the determination. As noted in the process analysis, claims examiners did not deny benefits if claimants said that they had not received the mailed notice to report for orientation; instead, they referred claimants to orientation and denied benefits only when they did not attend this initial service.

One further question that can be raised about the nonmonetary determination and denial process is whether the additional determinations performed for treatment members were concentrated among individuals who did not participate in the various mandatory services. This question is addressed

TABLE V.7

NONMONETARY NONSEPARATION ISSUES AND THEIR OUTCOMES

(Percent)

	Treatment Group			
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group
Issues Considered				
Availability	20.6	19.1	18.2	21.4
Ability	6.5	5.4	6.6	7.2
Active search for work	3.4	3.5	5.0	4.8
Unsuitable work	1.4	1.5	1.6	2.2
Reporting requirements	24.9	26.9	26.1	13.0
Less than 7 days[a]	23.9	24.3	22.0	26.7
Pension offset	8.6	10.3	9.6	13.6
Other	10.8	9.2	10.9	11.3
Probability That More Than One Issue Is Considered	24.3	23.0	22.8	26.6
Probability of Denial[b]				
Availability	43.5	47.1	44.5	48.8
Ability	76.0	77.5	77.8	73.7
Active search for work	46.2	39.4	45.5	44.7
Unsuitable work	18.8	10.7	23.5	17.7
Reporting requirements	20.9	23.9	22.3	44.1
Less than 7 days	100.0	99.8	100.0	100.0
Pension offset[c]	1.0	0.5	0.0	0.9
Other	68.6	56.6	59.2	94.4
Sample Size (Number of Issues)	1,153	1,901	1,098	787

NOTE: The analysis is restricted to the first two nonmonetary determinations reported for each individual for the current benefit year. The data pertain to the first three issues reported for the first nonseparation issue determination.

[a] New Jersey's unemployment insurance program requires that claimants be eligible for the full calendar week to receive a benefit. When claimants are ineligible for any day during a calendar week, they are denied benefits for the full week. These issues are termed "less than 7 days."

[b] The probability of a denial is the percentage of determinations that lead to a denial.

[c] The pension offset determinations reported here reduce weekly UI benefits but do not lead to a denial. Thus, they are not shown as denials in Table V.5.

in Table V.8, which shows data on the likelihood of a determination or denial for treatment group members who collected more than 5 weeks of benefits. This sample restriction is used to focus that analysis on claimants who in most cases should have had a determination if they did not attend scheduled services. The data in the table clearly show that the probability of having a nonmonetary determination or denial was higher for claimants who did not attend orientation or other services than for those who did attend. However, the determination rates were roughly 50 percent for those who did not attend, suggesting that there may have been some slippage in the process of identifying and examining the eligibility of claimants who did not participate in the demonstration's mandatory services. However, further analysis of this issue indicates that the compliance process worked well. Among those who did not attend the scheduled orientation and continued to collect UI, approximately 90 percent either had a nonmonetary determination or were excused from the demonstration reporting requirements because they moved or had a gap in their period of UI collection.[17]

In summary, the increased determination and denial rates experienced by the treatment groups provide evidence that the compliance process was likely an important contributing factor to the impacts of the treatments on UI receipt. The increased denials probably had some direct negative impact on UI receipt although the indirect effects of the compliance process were probably more important.

---

[17] See further discussion in Chapter VIII of the "Implementation and Process Report."

TABLE V.8

NONSEPARATION ISSUE DETERMINATIONS AND DENIALS BY PARTICIPATION IN INITIAL SERVICES[a]

	Percent with a:	
	Determination	Denial
Orientation		
Attended	40.7	18.2
Did not attend	51.8	23.0
Job-Search Workshop		
Attended	40.9	18.5
Excused	37.0	14.8
Did not attend	51.2	24.0
Assessment/Counseling		
Completed	40.6	17.9
Did not complete	45.4	20.8
Sample Size	7,451	7,451

---

[a] The sample for these tabulations consists of treatment members who collected more than five weeks of benefits. The five-week restriction was used to focus the analysis on claimants who should have had determinations if they did not attend scheduled services.

VI. IMPACTS ON EMPLOYMENT AND EARNINGS

In general, the treatments in the NJUIRDP were expected to promote the rapid reemployment of claimants, and thus to have a positive impact on the employment and earnings of claimants following their entry into the UI system. Thus, a fundamental issue in the evaluation of the reemployment demonstration is whether such effects occurred. This chapter addresses this issue by examining the employment and earnings of claimants during the year following their initial UI claim. In addition, the characteristics of the first post-UI job are examined to determine whether the treatments had any effect on the types of jobs obtained by claimants. This is an important issue, since it is possible that, by promoting rapid reemployment, the treatments might have led claimants to accept jobs that were less desirable that those obtained by claimants who were not offered special services.

Section A of this chapter considers the general impacts of the services provided through the demonstration on the employment and earnings of UI claimants in the three treatments. The analysis also investigates the impacts of the treatments on demographic subgroups of the claimant population. Section B then evaluates the effect of the demonstration treatments on the length of time from the initial UI claim to reemployment. In the final section, we use data derived from the claimant interview to investigate the impact of the treatments on various characteristics of the first post-UI job, including a discussion of the wage rate received, the hours worked, and the occupation held in that job.

A. THE IMPACTS OF THE TREATMENTS ON EMPLOYMENT AND EARNINGS

In this section we examine impacts on employment and earnings. We begin by discussing the two sources of data available for this analysis--interview data and wage-records data. We then present our main estimates of the impacts of the three demonstration treatments on employment and earnings. These estimates are drawn from the interview data, since we believe that these data provide better estimates of the impacts of the treatments than do the wage-records data. However, we also present the wage-records impact estimates in the last subsection, since they provide an alternative set of impact estimates.

1. Data Sources and Issues

Two sources of data on employment and earnings were available and could have been used for this analysis. The first data source is the follow-up interview, which contains the responses of individuals to questions about their employment and earnings experience. The second data source consists of administrative data from the state wage-records files, which provide a measure of each claimant's earnings and weeks worked in UI-covered employment by calendar quarter. These data are reported by employers on a quarterly basis.

Both the interview data and the records data have drawbacks for the analysis. Data extracted from the interview responses fall prey to the same measurement error problems which plague any interview data set, such as faulty recall by respondents about the dates of employment and wage rates. Moreover, nonresponse to the survey could bias the results, although, as we show in Appendix B, nonresponse does not appear to be a serious problem with this survey. The wage-records data set, on the other hand, does not capture all earnings received by claimants. Employment found by workers outside the state of New Jersey[1] and employment outside the UI-covered sector, including self-employment,[2] are not included. The wage-records data set is also organized by calendar quarter, and more disaggregated time units cannot be used for analysis.

This latter situation presents, we believe, a major problem with using the wage-records data for this evaluation, since, as we showed in the previous chapter, much of the impact of the treatments appears to have occurred early in the claim spell when the treatments were introduced. One cannot use the wage-records data to focus very precisely on this time period, since the calendar quarter that contains the initial UI claim is likely to contain earnings data from the pre-UI job in addition to post-UI earnings. Moreover, the pre-UI earnings are likely to swamp any post-UI earnings, making it difficult to detect the impacts of the treatments. Since this problem can be avoided with the interview data by measuring the impacts from the date of the initial claim, we performed our main analysis based on the interview data. However, the wage records were used for a subsidiary analysis to provide an alternative set of impact estimates.[3]

2. Impacts on Employment and Earnings by Treatment Group

Our analysis of the impacts of the NJUIRDP on employment and earnings focused on two principal measures: (1) the proportion of time employed in each quarter following the claim filing date and (2) earnings per quarter. The probability of having a job during a quarter was also examined, but this measure of employment receives less attention in our discussion. These measures of employment and earnings were derived by using a respondent's reported beginning and ending dates and wage rates for each job held after the claim filing date.

---

[1] Employment outside the state may be important in the case of New Jersey because of the state's proximity to the large labor markets of the New York, Philadelphia, and Wilmington areas.

[2] About 3 percent of the eligible population reported on the interview that they were self-employed at some point after the claim filing date.

[3] A comparison of the wage records and interview data on earnings is presented in Appendix D.

The means for these employment and earnings measures are reported in Table VI.1 for the first four quarters following the claim filing date and for the entire year following that date.[4] The data on the proportion of time employed indicate that the claimants in the demonstration were employed for somewhat less than half of the year following the claim filing date. The quarterly figures show that rates of employment increased across time for each of the treatment and control groups, with the largest increase occurring between the first and second quarters. The earnings data show that the claimants earned approximately $8,000 to $9,000 over the year following their claim filing date. Earnings by quarter grew rapidly between the first and third quarters following the claim filing date, as did employment, with noticeable but more gradual increases in the fourth quarter.

The data in the table suggest that each of the treatments may have had positive effects on employment and earnings, since measured employment and earnings appear to be higher for the treatment group than for the control group. To examine these apparent impacts further and to provide estimates of these impacts, we used the regression model approach discussed in the previous chapter. We estimated models that used the quarterly values for the percent of time employed and earnings as the dependent variables, and used the same independent variables on claimant characteristics, office, and time as were used for the UI analysis. Both overall impacts and impacts by population subgroup were estimated.

In the next three subsections of this chapter, we use the estimated coefficients from these employment and earnings regressions to calculate the impacts of the treatments on the employment and earnings of UI claimants. Each of these subsections considers the impacts of one of the three treatments provided in the NJUIRDP--job-search assistance (JSA) only, JSA plus training and relocation, and JSA plus the reemployment bonus.

a. Job Search Assistance

The regression-based estimated impacts of job-search assistance (JSA) on employment and earnings are reported in Tables VI.2 and VI.3, respectively, for the first four quarters following the

---

[4] Later in this chapter, we examine quarters beyond a one-year period when we consider the effect of the JSA plus training or relocation treatment, which was expected to have longer-run impacts than the other treatments.

TABLE VI.1

EMPLOYMENT AND EARNINGS OUTCOMES BY TREATMENT GROUP: SURVEY DATA

(Standard Errors of Means in Parentheses)

	Treatment Group			
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control
Mean Percent of Time Employed				
Quarter 1	16.6 (0.8)	16.1 (0.6)	17.1 (0.8)	14.2 (0.7)
Quarter 2	44.5 (1.4)	42.6 (1.1)	44.9 (1.4)	39.6 (1.2)
Quarter 3	60.6 (1.4)	58.6 (1.1)	59.1 (1.4)	56.2 (1.2)
Quarter 4	65.8 (1.5)	64.8 (1.2)	64.0 (1.4)	62.9 (1.3)
Year 1	47.1 (1.2)	45.3 (0.9)	45.8 (1.1)	42.8 (1.0)
Probability of Employment				
Quarter 1	34.2	33.7	36.1	30.3
Quarter 2	54.7	53.6	54.7	50.7
Quarter 3	67.3	65.7	66.2	64.9
Quarter 4	71.8	70.5	69.5	70.2
Year 1	73.8	73.7	72.1	72.6
Mean Earnings (dollars)				
Quarter 1	810 (50)	758 (37)	867 (51)	687 (40)
Quarter 2	2,189 (92)	2,014 (69)	2,270 (91)	1,945 (76)
Quarter 3	2,848 (97)	2,750 (75)	2,896 (96)	2,701 (85)
Quarter 4	3,021 (106)	3,048 (82)	3,097 (101)	3,012 (100)
Year 1	8,848 (327)	8,513 (244)	9,041 (314)	8,287 (282)
Sample Size	1,028	1,672	1,146	1,449

NOTE: The quarters are measured from the date of the initial UI claim.

TABLE VI.2

ESTIMATED IMPACT OF JSA ONLY ON THE
PERCENT OF TIME EMPLOYED

(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Overall Impact	2.3** (1.1)	4.2*** (1.8)	4.3** (1.8)	2.8 (2.0)
Percent of Control Mean	16.2	12.4	7.7	4.5
Females	3.8	6.4	5.5	2.7
Males	1.3	3.9	3.1	2.1
Age ≥ 55 Years	1.2	3.7	7.5	7.5
Age 35 - 54 Years	4.0	6.5	4.4	1.3
Age 25-34 Years	1.0	3.8	1.5	1.4
Blacks	3.7	6.9	2.8	0.6
Whites	0.5	4.5	4.5	2.5
Other Races	6.9*	5.1	4.5	4.8
Union Hiring Hall Users	7.2	5.2	3.0	-1.4*
Did Not Use Union Hiring Hall	2.2	5.1	4.3	3.0
Expected Recall	5.6*	10.4*	10.8**	9.2**
Did Not Expect Recall	0.7	2.1	0.5	-0.9
Durable Manufacturing	0	1.4	6.5	3.6
Nondurable Manufacturing	3.6	10.4	9.3	6.5
Nonmanufacturing	2.8	4.5	1.9	1.2
Nongraduates	4.4	11.8	9.4	5.6
High School Graduates	2.8	3.4*	2.8	2.2
College Graduates	-0.5*	8.2	5.6	2.6

TABLE VI.2 (continued)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	3.5	5.4	6.0	4.9
No Prior Experience with UI	0.9	4.6	1.7	-0.4
Married, Spouse Employed	2.8	7.9	7.1	7.1*
Married, Spouse Not Employed	2.9	0.8	0.2	-3.0
Not Married	1.8	4.1	3.0	0.7
White Collar	5.1	8.8	8.3	7.9
Clerical	4.8	7.3	7.2	6.7
Blue Collar	0.4	2.5	1.2	-1.2
Quarter 1 Cohort	-0.6	4.1	1.9	-0.9
Quarter 2 Cohort	5.4	5.4	5.7	6.1
Quarter 3 Cohort	4.1	8.3	6.4	1.2
Quarter 4 Cohort	0.7	2.3	2.7	4.2

NOTE: The quarters are measured from the date of the initial UI claim. The impacts were estimated using the interview sample. (For the regressions, see Appendix D.) The statistical tests shown for the subgroup impacts test for differences within categories. For example, impacts for males are compared with impacts for females. For characteristics with more than two categories, the tests are, respectively, comparisons with ages 35 to 54, whites, nonmanufacturing, nongraduates, not married, blue collar and the quarter 4 cohort.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE VI.3

ESTIMATED IMPACT OF JSA ONLY ON EARNINGS
(Dollars)
(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Overall Impact	125** (63)	263** (111)	171 (116)	49 (129)
Percent of Control Group Mean	18.2	13.5	6.3	1.6
Females	119	157	94	-177
Males	141	357	233	142
Age ≥ 55 Years	71	40	16	-16
Age 35 - 54 Years	262	484	406	167
Age 25-34 Years	-36*	69	-107*	-291
Blacks	252	395	135	37
Whites	39	275	227	-40
Other Races	289	120	23	33
Union Hiring Hall Users	560	502	261	69
Did Not Use Union Hiring Hall	102	246	160	-16
Expected Recall	317*	622**	612**	381*
Did Not Expect Recall	26	60	-83	-230
Durable Manufacturing	58	208	498	410*
Nondurable Manufacturing	92	502	456	356
Nonmanufacturing	164	201	-24	-253
Nongraduates	240	608	450	235
High School Graduates	121	132	49	-51
College Graduates	115*	712	547	107

TABLE VI.3 (continued)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	159	220	205	92
No Prior Experience with UI	90	321	112	-156
Married, Spouse Employed	192	489*	372	219**
Married, Spouse Not Employed	67	-62	-158	-601
Not Married	94	172	103	39
White Collar	301*	369	103	-6
Clerical	285	584	694*	580*
Blue Collar	-4	104	26	-205
Quarter 1 Cohort	43	149	-107	-100
Quarter 2 Cohort	189	111	194	197
Quarter 3 Cohort	237	604	385	118
Quarter 4 Cohort	45	180	182	-274

NOTE: The quarters are measured from the date of the initial UI claim. The impacts were estimated using the interview sample. (For the regressions, see Appendix D.) The statistical tests shown for the subgroup impacts test for differences within categories. For example, impacts for males are compared with impacts for females. For characteristics with more than two categories, the tests are, respectively, comparisons with ages 35 to 54, whites, nonmanufacturing, nongraduates, not married, blue collar and the quarter 4 cohort.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

claim filing date. The estimated impacts of the treatment on the proportion of time employed indicate that claimants who received JSA worked a greater percentage of time on average during each of the first four quarters following the claim filing date than did claimants in the control group, and these estimated impacts are statistically significant for the first three quarters. During the first quarter, the impact was 2.3 percentage points, which represents about a 16 percent increase in employment relative to the control group mean. In subsequent quarters, the point estimates are higher, but they represent a smaller percentage of the control group mean. Thus, it appears that this treatment was effective at encouraging rapid reemployment, which was its intention. Because it was not intended to promote long-term employment and earnings prospects, it is thus not surprising that the impact declined over time. These findings are consistent with those found with the UI data, which suggested that job-search assistance had an impact early in the unemployment spells of claimants.

The earnings impacts reported in Table VI.3 are, as one would expect, consistent with the employment impacts. They demonstrate that job-search assistance increased the earnings received by claimants following the claim filing date. Claimants who were offered job-search assistance earned $125 more in the first quarter, on average, than did claimants in the control group, an increase of approximately 18 percent. Job-search assistance significantly increased earnings for claimants in the first and second quarters, while the estimated impacts for the third and fourth quarters were positive but not statistically significant. Like the estimated impacts on employment, the impacts on earnings declined over time.

The impacts of job-search assistance on quarterly earnings in percentage terms were nearly equal to the employment impacts for the corresponding quarters. Thus, the earnings impacts appear to have been driven primarily by the impacts on employment, rather than by impacts on hourly wage rates (the impacts of the treatments on wage rates are considered explicitly later in this chapter).

Tables VI.2 and VI.3 also report the estimated impacts of job-search assistance on the employment and earnings of demographic subgroups of the claimant population, indicating significant differences between the subgroup impacts. Although few of the differences between subgroups were significant (in part, because of small sample sizes), some of the results are worth noting:

- Impacts for claimants who expected to be recalled were greater than for those who did not.
- Impacts for clerical workers and white-collar workers exceeded those for blue-collar workers.
- Impacts were greater for nongraduates than for college or high school graduates.

- o Impacts, particularly on earnings, were larger for the middle-age group than for the younger or older age groups.
- o Impacts by industry were greatest for nondurable goods manufacturing.

Only the greater impact for workers who expected to be recalled was consistently significant over time.

These subgroup findings are generally consistent with the estimated impacts on UI receipt by subgroup that were presented in Chapter V. Claimants with marketable skills, such as clerical workers and white-collar workers, appear to have experienced greater impacts than did individuals who faced long-term, structural unemployment problems, such as blue-collar workers, workers from durable-goods manufacturing industries, and permanently separated workers. This conclusion implies that the displaced with more severe reemployment problems may have been less affected by this treatment than were workers who faced relatively more favorable reemployment prospects.

One subgroup finding that requires further investigation is that individuals who expected to be recalled were affected by the demonstration treatments. This may seem counter-intuitive, since recall decisions are made by employers, not by claimants. Thus, the mechanism by which this treatment increased employment rates for this group is not readily apparent. The answer appears to pertain to the fact that not all individuals who expect to be recalled but who do not have a definite date of recall (individuals with a definite date of recall were not demonstration-eligible) are actually recalled. Among the control group, this percentage was 50.3 percent; for treatment 1, it was 46.4 percent. Moreover, the percentage of treatment 1 claimants who expected to be recalled and who did not have a job as of the interview date was lower than the percentage observed for the control group (15.5, compared with 18.3 percent, respectively). Thus, this treatment appears to have increased employment among those who expected to be recalled by increasing the probability that they would accept a new job as opposed to waiting until they were recalled.[5]

b. JSA Plus Training and Relocation

Our expectations about the impact of the JSA plus training or relocation treatment on employment and earnings were less certain than for the other treatments. Individuals who did not enter training were expected to experience increased employment and earnings because of the job-search assistance provided by the treatment. However, individuals who enrolled in training were not expected to experience such impacts in the short-run, since they remained unemployed for the duration of their training. The benefits of training were expected to appear eventually as enhanced employment stability and possibly increased earnings.

---

[5] Data on the amount of time spent looking for a job do not show that the treatment affected the intensity of job search among those who expected to be recalled; however, it may have affected the likelihood that they would accept a job offer.

Tables IV.4 and VI.5 present the impacts of this treatment on employment and earnings. As can be seen, the impacts were positive in each quarter, but smaller than the impacts for job-search assistance alone that were discussed in the previous section. For this reason, fewer of the estimated impacts are statistically significant (the impacts for the first two quarters for the percent of time employed are significant). In addition, as we found for the impacts of JSA, the impacts of treatment 2 decreased over time--for example, decreasing for the percent of time employed from a maximum of 13.4 percent of the control group mean to a minimum of 2.7 percent in the fourth quarter.

Tables VI.4 and VI.5 also show that, for the most part, the impact of treatment 2 did not vary significantly by demographic subgroup. One exception was the education subgroups, where treatment 2 increased employment more among high school graduates and nongraduates than among college graduates. A similar distinction between earnings impacts existed for these subgroups, though it was not statistically significant. The subgroup impacts of treatment 2 thus provide us with little additional information.

An important question about treatment 2 is whether the provision of training to some individuals in this treatment had a positive effect on their employment and earnings. As we noted above, impacts on employment and earnings were generally larger for the JSA-only treatment than for the JSA plus training or relocation treatment throughout the first four quarters after the claim filing date,[6] so we can conclude that the additional offer of training did not have a positive impact on employment and earnings over this period. However, since we expected that the impact of training would occur only in the longer run after training was completed, this finding does not mean that training had no impacts. To investigate the possibility of longer-run impacts, we examined earnings in quarters 5 and 6.[7] This investigation also showed no impacts (the point estimates for earnings were $2 in quarter 5 and -$124 in quarter 6, and neither impact was statistically significant). However, this finding should also not

---

[6] There is one exception--the point estimate for the treatment 2 impact on earnings in the fourth quarter was larger than the treatment 1 impact--but in that instance the impacts for both treatments were small and not statistically significant.

[7] Sample sizes for quarters 5 and 6 were substantially smaller than for quarters 1 to 4. The sample size (treatments plus controls) was 3,033 for quarter 5, about 60 percent of the sample available for the analysis of quarters 1 to 4, and 1,301 for quarter 6, about 25 percent of the sample available for analysis of quarters 1 to 4.

TABLE VI.4

ESTIMATED IMPACT OF JSA PLUS TRAINING/RELOCATION ON THE PERCENT OF TIME EMPLOYED

(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Overall Impact	1.9**	2.8*	2.2	1.7
	(0.9)	(1.6)	(1.6)	(1.7)
Percent of Control Group Mean	13.4	7.0	3.9	2.7
Females	3.0	5.8	4.8	2.5
Males	1.4	0.8	0.5	1.4
Age $\geq$ 55 Years	1.9	7.3	6.8	1.8
Age 35 - 54 Years	2.3	1.9	0.8	1.0
Age 25-34 Years	2.1	2.2	2.4	3.3
Blacks	3.9	8.9*	0.6	3.0
Whites	0.7	0.4	1.4	-0.6
Other Races	4.8	6.4	7.4	8.0
Union Hiring Hall Users	8.5	4.1	0.6	-10.7*
Did Not Use Union Hiring Hall	1.7	3.1	2.7	2.7
Expected Recall	3.8	5.8	2.1	1.8
Did Not Expect Recall	1.3	1.7	2.8	2.0
Durable Manufacturing	1.4	0.2	0.6	1.6
Nondurable Manufacturing	1.4	6.2	3.3	3.8
Nonmanufacturing	2.6	3.1	3.0	1.4
Nongraduates	3.6	3.4	-1.9	-1.2
High School Graduates	2.8	4.7	4.9	2.9
College Graduates	-1.7**	-4.5*	-5.9	-0.6

TABLE VI.4 (continued)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	2.8	3.7	3.8	4.0
No Prior Experience with UI	1.2	2.4	0.8	-1.0
Married, Spouse Employed	2.5	2.4	1.4	2.4
Married, Spouse Not Employed	1.5	0.8	0.3	1.4
Not Married	2.1	5.3	5.2	1.6
White Collar	4.1	6.9	4.9	2.9
Clerical	3.1	1.5	-1.6	4.9
Blue Collar	0.9	1.9	2.8	0.4
Quarter 1 Cohort	1.1	1.1	-1.7	-1.6
Quarter 2 Cohort	2.7	6.9	6.4	4.8
Quarter 3 Cohort	1.3	2.5	4.2	3.4
Quarter 4 Cohort	3.2	1.6	0.4	0.3

NOTE: The quarters are measured from the date of the initial UI claim. The impacts were estimated using the interview sample. (For the regressions, see Appendix D.) The statistical tests shown for the subgroup impacts test for differences within categories. For example, impacts for males are compared with impacts for females. For characteristics with more than two categories, the tests are, respectively, comparisons with ages 35 to 54, whites, nonmanufacturing, nongraduates, not married, blue collar and the quarter 4 cohort.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE VI.5

ESTIMATED IMPACT OF JSA PLUS TRAINING/RELOCATION
ON EARNINGS
(Dollars)
(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Overall Impact	82 (55)	103 (98)	83 (103)	77 (112)
Percent of Control Group Mean	11.9	5.3	3.1	2.6
Females	121	185	223	2
Males	75	64	-14	51
Age $\geq$ 55 Years	28	120	134	-86
Age 35 - 54 Years	118	110	78	77
Age 25-34 Years	113	142	107	33
Blacks	174	147	-175	-133
Whites	40	39	86	-44
Other Races	196	332	353	354
Union Hiring Hall Users	461	216	101	-393
Did Not Use Union Hiring Hall	73	116	99	56
Expected Recall	162	276	37	-34
Did Not Expect Recall	60	35	134	63
Durable Manufacturing	74	10	211	374
Nondurable Manufacturing	21	108	58	176
Nonmanufacturing	128	159	79	-122
Nongraduates	166	168	-56	-43
High School Graduates	112	162	142	44
College Graduates	-5	-35	68	162

TABLE VI.5 (continued)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	142	130	156	103
No Prior Experience with UI	33	109	18	-79
Married, Spouse Employed	134	175	135	96
Married, Spouse Not Employed	99	60	-40	-293
Not Married	53	93	132	120
White Collar	152	225	14	-29
Clerical	84	-8	-65	140
Blue Collar	74	113	195	19
Quarter 1 Cohort	18	30	-236	-210
Quarter 2 Cohort	180	293	325	330*
Quarter 3 Cohort	65	172	267	237
Quarter 4 Cohort	94	-30	-27	-339

NOTE: The quarters are measured from the date of the initial UI claim. The impacts were estimated using the interview sample. (For the regressions, see Appendix D.) The statistical tests shown for the subgroup impacts test for differences within categories. For example, impacts for males are compared with impacts for females. For characteristics with more than two categories, the tests are, respectively, comparisons with ages 35 to 54, whites, nonmanufacturing, nongraduates, not married, blue collar and the quarter 4 cohort.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

be considered conclusive, since relatively few individuals in this treatment were placed in training, and it is thus difficult to detect the impacts of training unless they are quite large.[8] Moreover, the data from the interview indicated that about 14 percent of the individuals who received classroom training were still unemployed at the time of the interview, in part because they had only recently completed training or were still in training. Thus, a longer time period may be necessary to measure any impacts of training.[9]

c. JSA Plus the Reemployment Bonus

Treatment 3 combined job-search assistance with the offer of a reemployment bonus to workers who found jobs within 11 weeks after the offer was made. We expected that treatment 3 would have the greatest impact on employment because of the additional reemployment incentives created by the bonus offer. We were less certain about its impacts on earnings, because the incentives created by the bonus might have led workers to sacrifice earnings in an effort to find a job quickly and collect the bonus, especially given that the bonus declined substantially over time.[10] The impacts of the combination of job-search assistance and the reemployment bonus offer on employment and earnings are reported in Tables VI.6 and VI.7. These estimates show that treatment 3 increased the percentage of time employed in quarters 1 and 2 by 19.7 and 12.6 percent, respectively. Earnings increased by 23.3 and 14.3 percent in the two quarters. These impacts were larger than those found for job-search assistance alone, and they are significantly different from zero at the 99 percent confidence level. The impacts of treatment 3 in the last two quarters declined rather sharply and are not statistically significant. In addition, these impacts were somewhat smaller than those estimated for job-search assistance alone.

Several differences by demographic subgroup existed for impacts on both employment and earnings in the first two quarters following the claim filing date:

- Impacts on the earnings of males were greater than impacts on the earnings of females in quarter 1.

---

[8] In addition, about 16 percent of the individuals who entered classroom training did not complete training. The two primary reasons cited by respondents for not completing training were that they became reemployed and that they could not afford to continue.

[9] Data from the interview indicate that about two-thirds of the claimants who received classroom training, and who had completed it long enough ago to have an opinion, felt that it had been useful to them in finding a job.

[10] In Section C of this chapter, we address the nature of the jobs obtained through this treatment.

TABLE VI.6

ESTIMATED IMPACT OF JSA PLUS REEMPLOYMENT BONUS ON THE PERCENT OF TIME EMPLOYED

(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Overall Impact	2.8*** (1.0)	5.0*** (1.7)	2.3 (1.8)	0.6 (1.9)
Percent of Control Mean	19.7	12.6	4.1	1.0
Females	1.2	5.3	3.5	1.6
Males	4.1	3.7	0.5	-1.2
Age $\geq$ 55 Years	1.0*	3.1	3.4	2.3
Age 35 - 54 Years	5.7	8.7	4.2	-0
Age 25-34 Years	-0.7**	-1.5**	-2.7	-1.2
Blacks	5.7	11.3	3.6	-0.9
Whites	1.9	2.4	0.5	-1.6
Other Races	2.6	4.7	4.6	6.0
Union Hiring Hall Users	3.5	5.8	-2.1	-9.8
Did Not Use Union Hiring Hall	2.7	4.4	2.2	0.8
Expected Recall	3.7	8.0	5.8	2.3
Did Not Expect Recall	2.2	2.5	-0.2	-1.1
Durable Manufacturing	1.0	-2.8	-3.0	-1.5
Nondurable Manufacturing	0.2*	11.5*	5.3	4.1
Nonmanufacturing	1.7	4.0	1.9	-1.6
Nongraduates	-0.1	2.9	-0.5	-2.0
High School Graduates	5.5**	6.5	3.9	1.3
College Graduates	-8.3***	-4.2	-6.7	-3.8

TABLE VI.6 (continued)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	2.5	3.1	1.0	1.8
No Prior Experience with UI	3.0	6.3	3.3	-2.1
Married, Spouse Employed	0.5	0.4*	-0.5	-1.3
Married, Spouse Not Employed	3.6	9.6	6.6	3.2
Not Married	4.9	6.4	2.2	0.2
White Collar	4.9	8.1	4.6	3.3
Clerical	7.4**	3.7	-0.7	-1.9
Blue Collar	0.2	2.9	1.5	-0.7
Quarter 1 Cohort	3.2	6.7	1.5	-1.4
Quarter 2 Cohort	5.2	10.5*	8.7*	8.4
Quarter 3 Cohort	0.5	-0.6	-0.9	-4.1
Quarter 4 Cohort	2.5	2.3	-0.9	-1.4

NOTE: The quarters are measured from the date of the initial UI claim. The impacts were estimated using the interview sample. (For the regressions, see Appendix D.) The statistical tests shown for the subgroup impacts test for differences within categories. For example, impacts for males are compared with impacts for females. For characteristics with more than two categories, the tests are, respectively, comparisons with ages 35 to 54, whites, nonmanufacturing, nongraduates, not married, blue collar and the quarter 4 cohort.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE VI.7

ESTIMATED IMPACT OF JSA PLUS REEMPLOYMENT BONUS ON EARNINGS
(Dollars)
(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Overall Impact	160*** (60)	278*** (107)	131 (113)	22 (124)
Percent of Control Mean	23.3	14.3	4.9	0.7
Females	33	144	147	-54
Males	261*	310	51	-73
Age ≥ 55 Years	56	150	46	-140
Age 35 - 54 Years	268	435	235	38
Age 25-34 Years	37	-36*	-88	-171
Blacks	206	398	-5	-246
Whites	153	174	77	-89
Other Races	108	258	231	149
Union Hiring Hall Users	179	220	-77	-436
Did Not Use Union Hiring Hall	150	232	108	-39
Expected Recall	283	477	353	190
Did Not Expect Recall	78	93	-47	-206
Durable Manufacturing	47	-210	-211	-109
Nondurable Manufacturing	-170*	152	-59	95
Nonmanufacturing	99	230	116	-188
Nongraduates	-54	-33	-311	-275
High School Graduates	226*	277	175	9
College Graduates	10	356	129	-88

TABLE VI.7 (continued)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	149	124	21	-22
No Prior Experience with UI	156	383	204	-123
Married, Spouse Employed	5	-55**	-138*	-213
Married, Spouse Not Employed	283	638	477	-46
Not Married	250	342	164	98
White Collar	220	341	28	-86
Clerical	403*	302	23	-166
Blue Collar	37	153	154	-20
Quarter 1 Cohort	186	296	-143	-192
Quarter 2 Cohort	402	622*	533	544**
Quarter 3 Cohort	-96	-22	59	-312
Quarter 4 Cohort	128	67	23	-322

NOTE: The quarters are measured from the date of the initial UI claim. The impacts were estimated using the interview sample. (For the regressions, see Appendix D.) The statistical tests shown for the subgroup impacts test for differences within categories. For example, impacts for males are compared with impacts for females. For characteristics with more than two categories, the tests are, respectively, comparisons with ages 35 to 54, whites, nonmanufacturing, nongraduates, not married, blue collar and the quarter 4 cohort.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

- The impacts on employment and earnings for middle-age claimants exceeded those for the younger claimants.
- High school graduates were affected more by treatment 3 than were nongraduates and college graduates.
- The impacts of the treatment were lower for claimants with an employed spouse than for married claimants whose spouse was not employed.
- The impacts for clerical workers were greater than the impacts for blue-collar workers.

The subgroup impacts found here were similar to those found for job-search assistance. The estimated subgroup impacts for treatment 3 thus support the conclusion outlined in our discussion of the JSA-only impacts that claimants with marketable skills appear to have been affected by the treatments to a greater extent than were workers who faced more severe long-term, structural unemployment problems.

To evaluate the impact of the reemployment bonus component of treatment 3, we compared claimants in treatment 1 who were assessed with those in treatment 3 who were assessed, since the bonus offer was made at the time of assessment. The results of making this comparison are reported in Table VI.8. As can be seen, the impacts of the bonus offer on employment and earnings are not as strong as the estimated impacts on UI receipt; only the impact on earnings in the first quarter after the date of claim is statistically significant at the 90 percent confidence level.[11] Nevertheless, there is some weak evidence that the bonus offer led to increased earnings.

3. Impacts of the Treatments Based on Wage-Records Data

Wage-records data are used in this section to provide an alternative estimate of the impacts of the treatments on earnings over the year following the claim filing date. As noted earlier, the wage-records data have one serious shortcoming for this analysis--namely, they are available only by calendar quarter, and, for this reason, they cannot be disaggregated to examine the impacts that began with the entry of claimants into the UI system. This presents a problem for the analysis. If we use the calendar quarter that contains the date of the claim, both some pre-UI and post-UI earnings will be included. While random assignment to treatment and control groups should make pre-UI earnings equal among groups, the fact that pre-UI earnings are likely to exceed any post-UI earnings in this quarter means that any impacts of the treatments are likely to be swamped by statistically insignificant differences in pre-UI earnings. This, in fact, appears to occur. No significant differences among treatment

---

[11] This difference in results may arise because the variance of earnings is larger relative to its mean than the variance of UI receipt relative to its mean. As a result, statistically significant differences in earnings are harder to detect than differences in UI receipt, given a fixed sample size.

TABLE VI.8

ESTIMATED IMPACTS ON EMPLOYMENT AND EARNINGS OF OFFER OF REEMPLOYMENT BONUS ON CLAIMANTS WHO WERE OFFERED THE BONUS

(Standard Errors in Parentheses)

	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Percent of Time Employed	0.7	1.4	-1.8	-2.8
	(0.7)	(2.0)	(2.2)	(2.3)
Earnings (Dollars)	58*	90	-22	-58
	(33)	(113)	(137)	(149)

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

and control groups appear for this quarter. Thus, the analysis presented herein examines the impacts that began with the first calendar quarter following the claim filing date, but this analysis is likely to miss any impacts that occurred early in the unemployment spell. Since our UI analysis indicated that such impacts did occur, we believe that the wage-records-based impact estimates are lower-bound estimates of the impacts of the treatments, and that the interview-based estimates present a better picture of the impacts.[12]

The impacts on earnings that are estimated on the basis of wage-records data are reported in Table VI.9 for quarters beginning with the first calendar quarter after the claim filing date. As can be seen, the estimate for treatment 3 for the first quarter is statistically significant, and it is quite similar in magnitude to the estimate obtained from the interview data. However, all remaining estimates are considerably lower than we found using the interview data, and none is statistically significant. Thus, these data do not generally support our previous finding that each of the treatments had an impact on employment and earnings. Nevertheless, the point estimates of impacts are positive when summed over the year, and we use these as lower-bound estimates of the impacts of the treatments on earnings in our benefit-cost analysis.

B. THE IMPACTS OF THE TREATMENTS ON TIME TO REEMPLOYMENT

In the previous section of this chapter, we investigated the impacts of the treatments on the employment and earnings of UI claimants. Although we described in detail the extent of these impacts, we did not attempt to determine how the observed impacts occurred. This section deals with one possible source of treatment impacts by considering how the duration of a worker's initial spell of not working was affected by the treatment which he or she was offered.[13] We use the initial spell of not working as opposed to the unemployment spell, since it is difficult to determine whether and when individuals in the sample dropped out of the labor force.

Our analysis of the impacts of the treatments on the initial spell of not working was performed in two ways. First, we used our basic regression model with the spell length as the dependent

---

[12] Estimated impacts using the wage records data are also biased downward since the data were only available for New Jersey. Earnings outside the state were not obtained.

[13] We used the time from the initial UI claim to the first post-UI job as our measure of this spell, rather that the time from the end of the pre-UI job to the first post-UI job. We did so both because we believed that the end date of the pre-UI job was subject to more measurement error than was the UI claim filing date and because we could compare the timing of the intervention with the spell by using the claim filing date. However, both measures were computed, and they both provided similar results.

TABLE VI.9

ESTIMATED IMPACTS OF TREATMENTS ON EARNINGS:
WAGE RECORDS DATA
(Dollars)
(Standard Errors in Parentheses)

Calendar Quarters Following the Date of Claim	Treatment Group		
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus
Quarter 1	28 (83)	58 (75)	176** (83)
Quarter 2	75 (90)	-23 (81)	79 (89)
Quarter 3	98 (82)	47 (75)	52 (82)
Quarter 4	-39 (93)	-10 (83)	16 (92)

*Statistically significant at the 90 percent confidence level for a two-tailed test.

**Statistically significant at the 95 percent confidence level for a two-tailed test.

***Statistically significant at the 99 percent confidence level for a two-tailed test.

variable to estimate the impacts of the treatments. This approach (see Table VI.10) suggests that each of the treatments reduced the time between the UI claim filing date and reemployment. Respondents from treatment group 1 averaged 2.1 weeks less in the initial period of not working than did respondents from the control group--a difference which represents about 7 percent of the mean weeks of duration for the control group (31 weeks), and which is statistically significant at the 95 percent confidence level. The estimates for the other two treatment groups showed an approximately one-week reduction in duration, but neither result was statistically significant. These estimates should be viewed with caution, however, since the full spell length was not observed for individuals who were still not working as of the interview date (which occurred for about 20 percent of the sample). This censoring of the spell length may bias these estimates of the impacts on duration, particularly if, as is the case, the censoring was more severe for the control group than for the treatment groups. This situation should lead to an underestimate of the true effects of the treatments.

Because of this problem, a second approach, which addresses the problem of censored observations of duration, was adopted for this analysis. This approach, which in recent years has come to be used widely in analyses of unemployment duration (see discussion in Kiefer, 1988), entails computing weekly rates of exit from the not-working status to employment. The distributions of these exit rates can be compared to determine whether they differ for different sample groups. A closely related measure is the survival rate, which measures at each point in time the probability that a randomly chosen respondent from a given sample is not working past that point in time.[14]

The estimated weekly survival rates for each treatment group are presented in Figure VI.1. As can be seen, the control group appears to have had the highest survival rate for all weeks during the first year following the claim filing date, implying that a greater proportion of the workers in the control group were still not working at any point in time relative to the three treatment groups. The results of statistical tests of the differences between the estimated survival functions for treatment and control samples were mixed,[15] but the results were strong enough to suggest that the treatments were effective at reducing the time between the UI claim filing date and reemployment.

---

[14] This methodology was also used to analyze UI duration. (See Chapter V for references and further discussion.)

[15] We used both the log rank tests of homogeneity of exit behavior between treatment groups and the Wilcoxon test. The log rank tests did not show statistically significant differences between groups, while the Wilcoxon tests did.

TABLE VI.10

ESTIMATED IMPACTS OF TREATMENTS ON TIME TO REEMPLOYMENT

(Standard Errors in Parentheses)

	Treatment Groups		
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus
Impact on Not Working Duration (Weeks)	-2.13** (0.97)	-0.86 (0.85)	-0.96 (0.93)

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

Comparisons of the survival-rate plots in Figure VI.1 by treatment group show some differences among the treatments. For example, treatment 2 appears to show smaller impacts relative to the control group over the first half of the year than is true of the other treatments. However, since most of the differences among treatments appear in the tail of the distribution (where sample sizes are small), we should not attempt to draw too many conclusions from these differences. Instead, it is perhaps more interesting to examine the exit rates shown in Figure VI.2. While it is difficult to interpret these plots since they jump around to a large extent, two points are worth mentioning. First, the impact of the treatments that we observed early in the unemployment spells of claimants based on the UI data (see Chapter V) is also apparent in Figure VI.2. That is, in general, the exit rates for the treatments appear to be higher than those for the control group over the first 12 to 16 weeks after the claim filing date. Second, the exit rates for both treatments and controls are high both initially and again in the 26- to 29-week range. The first peak period represents a time at which a substantial portion of the sample was able and willing to find a job quickly. The second peak period is likely to be caused by the exhaustion of UI benefits, a point at which many workers must find a new job to avoid the severe financial hardship from the loss of benefits, and some workers take jobs which they avoided while UI benefits were still available.

The results summarized in this section imply that the impacts of the treatments on the claimants' initial spells of not working were probably responsible for the majority of the impacts of the treatments on employment and earnings which were discussed earlier. The impacts for at least treatment 1 were relatively large, reducing initial spells of not working by as much as two weeks or more, and statistically significant. The magnitude of this impact is quite consistent with the employment impacts cited earlier. For example, the estimates in Table VI.2 aggregated over the entire year indicate that the estimated impact of treatment 1 was to increase the time of employment by 3.6 percent. Multiplying this impact by 52 weeks yields an impact of 1.9 measured in weeks. This result is close to the least squares estimate of the impact of treatment 1 on unemployment duration presented in Table VI.10.[16] While the consistency between these results is not surprising, given that both employment measures were derived from the same survey responses, it does demonstrate that it was primarily the initial spell of unemployment duration which was affected by the treatment. It is also comforting to know that we derived similar employment impacts for the demonstration treatments when we used different dimensions of measurement and methods of estimation.

---

[16] Performing the same calculations for the other treatment groups yields a similar conclusion.

FIGURE VI.1
DURATION TO FIRST POST-UI JOB - SURVIVAL RATES

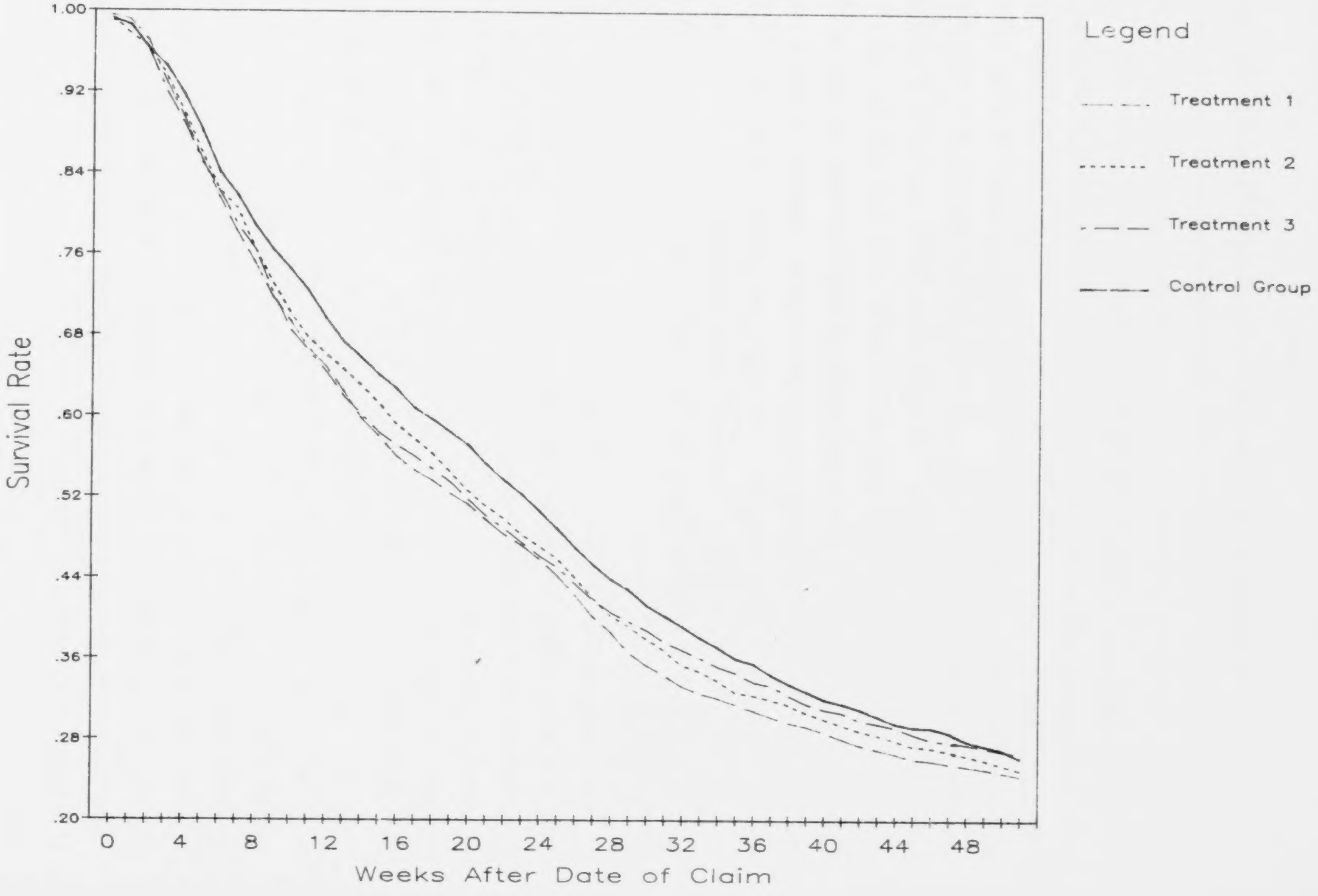
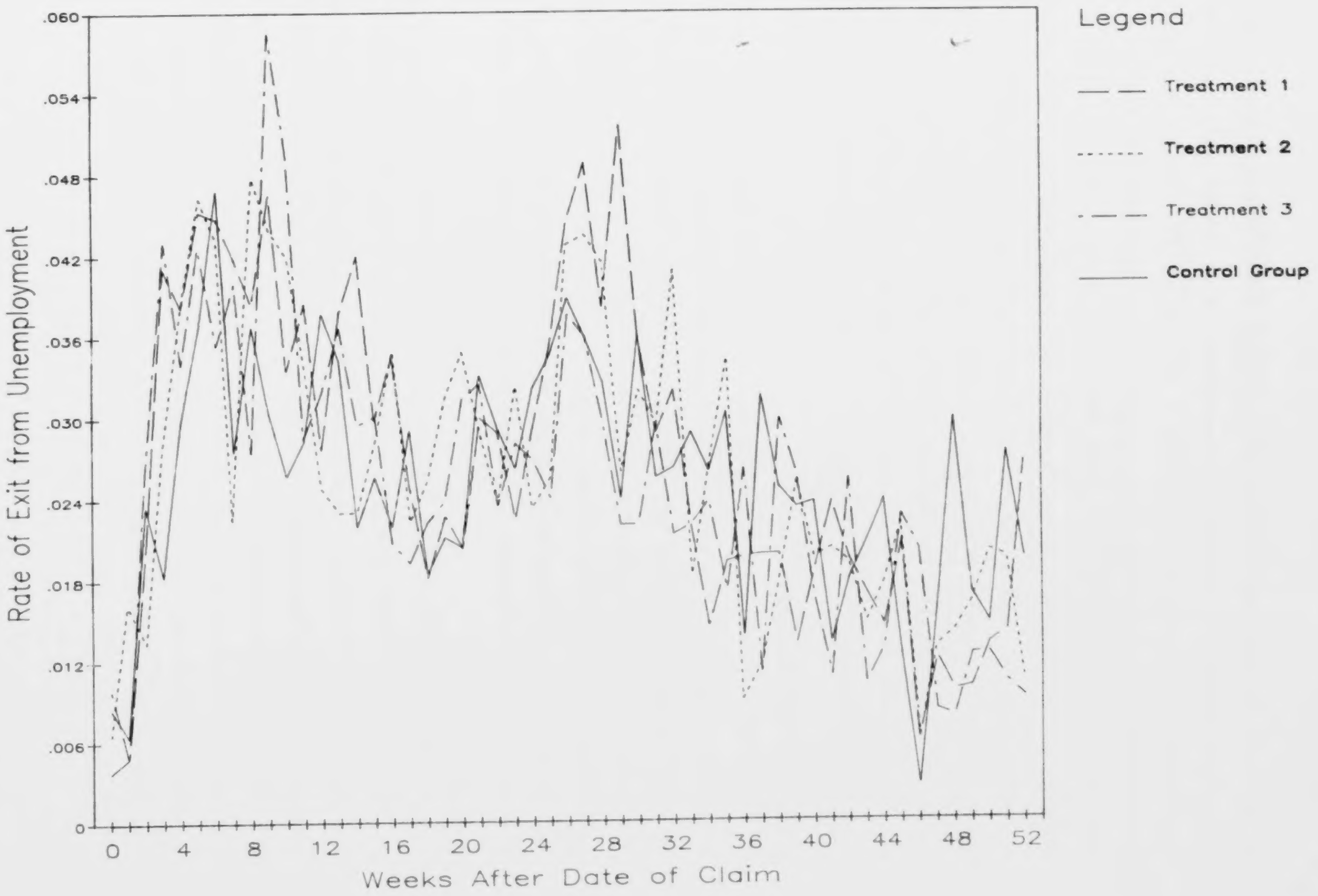


FIGURE VI.2
DURATION TO FIRST POST-UI JOB - EXIT RATES



C. IMPACTS OF THE TREATMENTS ON THE FIRST POST-UNEMPLOYMENT JOB

Earlier sections of this chapter have demonstrated that the program treatments appear to have been effective at encouraging the reemployment of UI claimants. The possibility exists, especially for the reemployment bonus treatment, that the claimants in the treatment groups who returned to the work force quickly sacrificed earnings on their jobs to hasten their reemployment. For example, job-search assistance may have encouraged claimants to move to lower-paying occupations in which openings could be found more readily. Workers who were offered the reemployment bonus in treatment group 3 may also have taken a low-paying job to obtain a substantial reemployment bonus. However, job-search assistance could also have improved the job-search skills of claimants, and they may have found better jobs than they would have otherwise. These issues are addressed in this section by examining hourly wage rates and hours worked for those employed. Changes in occupations are also addressed.

1. Post-UI Hourly Wage Rates

Table VI.11 presents information on the mean pre- and post-UI hourly wage rates for individuals who became reemployed.[17] The mean pre- and post-UI wage rates declined for each of the treatment and control groups, indicating that many claimants experienced some decline in hourly wage rates due to their unemployment spell. Moreover, the decline in mean hourly wage rates was largest for the control group, suggesting that the treatments did not have an adverse effect on post-UI hourly wage rates. In fact, the treatments may have had a positive effect on post-UI hourly wages, since the decline in mean hourly wages was smaller for members of the treatment groups than for members of the control group.

To investigate whether the treatments had a positive effect on post-UI hourly wages, we estimated two regression models. One model used the post-UI wage rate as the dependent variable, and the other used the natural log of the wage rate. The natural log specification is often used in the economics literature for wage equations. The results of this exercise are also reported in Table VI.11. As can be seen, the estimated impacts were positive for all treatment groups, and they are statistically significant when the natural log is used (the impact of treatment 1 is also statistically significant when the dollar value of the wage rate is used). The coefficients for the natural log specification can be interpreted as the percentage change in the post-UI wage rate brought about by the treatments.

---

[17] Hourly wage rates rather than weekly earnings are used in this analysis in order to compare compensation levels more accurately across jobs for a constant amount of effort.

TABLE VI.11

PRE- AND POST-UI HOURLY WAGES
(Dollars)

(Standard Errors in Parentheses)

	Treatment Group			
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group
Means				
Pre-UI hourly wage	9.56 (0.18)	9.45 (0.14)	9.89 (0.16)	9.76 (0.16)
Post-UI hourly wage	9.49 (0.21)	9.21 (0.15)	9.58 (0.17)	9.22 (0.16)
Difference	-.07	-.24	-.31	-.54
Estimated Treatment Impact				
Post-UI wage rate	0.37* (0.20)	0.19 (0.18)	0.26 (0.20)	
Natural log of post-UI wage rate	0.041** (0.017)	0.030** (0.015)	0.041** (0.017)	
Sample Size	819	1,347	898	1,138

*Statistically significant at the 90 percent confidence level for a two-tailed test.

**Statistically significant at the 95 percent confidence level for a two-tailed test.

***Statistically significant at the 99 percent confidence level for a two-tailed test.

Thus, the estimated impacts are that treatments 1 and 3 increased hourly wage rates by 4 percent and treatment 2 by 3 percent.

In summary, our examination of hourly wage rates on the post-UI job shows no evidence that claimants in the treatment groups may have sacrificed wages to become reemployed quickly. Instead, there is some evidence that each of the treatments led to increased hourly wage rates on the post-UI job.

2. Work Hours in the Post-UI Job

Another dimension of post-UI jobs that may have been affected by the treatments is hours worked on the post-UI job. Although the treatments did not cause a decline in wage rates, it is possible that claimants who were offered the treatments may have sacrificed earnings by accepting a part-time job to facilitate reemployment.[18] Alternatively, by providing job-search assistance, the treatments may have enabled treatment group members to find full-time employment more readily than could members of the control group.

Data to investigate these possibilities are reported in Table VI.12. These data demonstrate that, in general, claimants worked fewer hours per week in their post-UI job relative to their pre-UI job. The decline was about 2 hours, from an average of 41 or 42 hours per week to an average of 39 or 40. However, the treatments did not reduce the mean number of hours worked significantly beyond this general reduction, nor in general, did they significantly increase the proportion of jobs that were part-time.[19]

3. Pre-UI and Post-UI Occupation

The final characteristic of the pre- and post-UI jobs that we examined is occupation. For treatments 1 and 3, we had no expectations about the impact of the treatments on occupation, but we did expect that treatment 2, because of the training, might lead to somewhat more occupational changes than would occur in the control group.

Data on the pre-UI and post-UI occupations of workers who found a post-UI job, and who reported both their pre-UI and post-UI occupation, are reported in Table VI.13 at the one-

---

[18] At least one of the treatments--the reemployment bonus--addressed this possibility by paying the bonus only to workers who found a full-time job.

[19] The JSA-only-control group difference in the probability of a part-time job is statistically significant at the 95 percent confidence level for a one-tail test, but the magnitude of the difference is small.

TABLE VI.12

PRE- AND POST-UI HOURS OF EMPLOYMENT PER WEEK

(Standard Errors in Parentheses)

	Treatment Group			
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group
Pre-UI Mean Hours Per Week	41.3 (0.29)	41.8 (0.23)	41.7 (0.30)	41.7 (0.23)
Post-UI Mean Hours Per Week	38.6 (0.36)	39.6 (0.29)	39.8 (0.32)	39.5 (0.27)
Difference of Means	-2.7	-2.2	-1.9	-2.2
Estimated Treatment Impact	-0.54 (0.42)	0.34 (0.37)	0.22 (0.41)	-- --
Percent Part-Time[a]	14.6	12.4	12.3	11.8
Sample Size	819	1,347	898	1,138

[a] A job was considered part-time if it was less than 32 hours per week. This definition was chosen because it was the definition used to determine if a job was eligible for the reemployment bonus.

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE VI.13

PRE-UI AND POST-UI OCCUPATION CODES (ONE-DIGIT LEVEL) FOR RESPONDENTS WHO FOUND A JOB BEFORE THE INTERVIEW

SOC Occupation Code	Percentage of Sample Pre-UI	Percentage of Sample Post-UI	Difference between Pre-UI and Post-UI Percentage
1	14.1	11.5	-2.6
2	2.7	3.0	0.3
3	3.9	3.8	-0.1
4	24.1	28.4	4.3
5	6.6	8.3	1.7
6	10.5	10.7	0.2
7	27.0	21.6	-5.4
8	11.1	12.7	1.6

NOTE: Occupation was coded using the standard occupation codes (SOC) found in the Standard Occupation Classification Manual, 1980. Washington, D.C.: U.S. Department of Commerce, Office of Federal Statistical Policy and Standards, 1980.

Occupation Definitions

1 Executive, Administrative, and Managerial Occupations
Engineers, Surveyors, and Architects
Natural Scientists and Mathematicians
Social Scientists

2 Lawyers, Social Workers, and Religious Workers
Teachers, Librarians, and Counselors
Health Diagnosing and Treating Practitioners
Nurses

3 Pharmacists, Dieticians, Therapists, and Physician's Assistants
Writers, Artists, Entertainers, and Athletes
Technologists and Technicians

4 Marketing and Sales Occupations
Administrative Support Occupations, including Clerical

5 Service Occupations
Agricultural, Forestry, and Fishing Occupations

6 Mechanics and Repairers
Construction and Extractive Occupations
Precision Production Occupations

7 Production Working Occupations

8 Transportation and Material Moving Occupations

digit occupation level. These data show that the dominant pre-UI one-digit occupations among these workers were occupation 4 (sales, administrative support, and clerical) and occupation 7 (production-working occupations). Post-UI percentages for each occupation are shown in the second column of the table. The same two occupations, 4 and 7, were also dominant after UI receipt.

The difference in occupations in percentage terms across time indicate that, following UI receipt, occupation 4 became more dominant, while occupation 7 became less dominant. These results are not unexpected, since New Jersey has been experiencing an expansion of employment opportunities in the clerical/administrative support field and a decline in employment opportunities for production jobs.[20]

Another way to examine occupational change is to examine retention rates by occupational category (i.e., the percentage of workers in each occupation who were able to find jobs in the same occupation after being unemployed). Data on retention rates (Table VI.14) indicate that occupation 4 (sales, administrative support, and clerical) had the highest rate of retention (78 percent). Again, this is not surprising, given the employment growth experienced for these occupations. Somewhat more surprising is that the retention rate for occupation 1, which includes executive, administrative, and managerial occupations, as well as engineers and scientists, was the lowest (57 percent). These occupations are presumably also in demand in the state. One reason for this difference in retention rates could be that employers may not use observed unemployment spells as a signal of a worker's quality to the same extent in occupation 4 as they do in occupation 1. Thus, employers may have been more willing to hire individuals in occupation 4 who were laid-off by other employers than they were to hire similar individuals in occupation 1. Another reason could be that the incidence of temporary layoffs may differ between the two occupations.

Table VI.14 also provides the retention rates for each occupation by treatment group. It should be noted that for five of the eight occupations the retention rate was highest for treatment group 3. The higher retention rate for this group suggests that workers who were offered a reemployment bonus may have been more inclined to take a job in their own field than were workers in the other treatments. Because they may have wanted to find a job quickly in order to receive a bonus, they may have looked for a job in the occupation with which they were most familiar to facilitate their transition to reemployment.

---

[20] One industry in New Jersey which has been a large contributor to the loss of production jobs is the apparel industry. Production-job losses in the demonstration sites partly reflect this decline.

TABLE VI.14

RETENTION RATES FOR ONE-DIGIT OCCUPATIONS

		Treatment Group			
SOC Occupation Code	Full Sample	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	Control Group
1	56.6	52.3	58.5	56.1	58.1
2	67.8	62.1	68.5	79.0	62.2
3	60.4	52.5	56.7	70.8	59.0
4	77.7	78.5	77.6	79.1	75.9
5	63.4	42.4	66.7	76.8	67.0
6	70.5	67.9	70.7	76.8	66.9
7	62.6	62.6	64.6	64.3	69.8
8	68.1	69.3	68.3	68.9	66.4
Sample Size	4,202	819	1,347	898	1,138

NOTE: Occupation was coded using the standard occupation codes (SOC) found in the *Standard Occupation Classification Manual, 1980*. Washington, D.C.: U.S. Department of Commerce, Office of Federal Statistical Policy and Standards, 1980.

Occupation Definitions

1 Executive, Administrative, and Managerial Occupations
Engineers, Surveyors, and Architects
Natural Scientists and Mathematicians
Social Scientists

2 Lawyers, Social Workers, and Religious Workers
Teachers, Librarians, and Counselors
Health Diagnosing and Treating Practitioners
Nurses

3 Pharmacists, Dieticians, Therapists, and Physician's Assistants
Writers, Artists, Entertainers, and Athletes
Technologists and Technicians

4 Marketing and Sales Occupations
Administrative Support Occupations, including Clerical

5 Service Occupations
Agricultural, Forestry, and Fishing Occupations

6 Mechanics and Repairers
Construction and Extractive Occupations
Precision Production Occupations

7 Production Working Occupations

8 Transportation and Material Moving Occupations

To consider the issue of occupations further, we constructed binary variables to indicate when respondents switched occupations at the one-digit or two-digit levels. These variables (Table VI.15) show that the means for the one-digit and two-digit indicators were, respectively, 0.34 and 0.47 for the entire sample, indicating that 34 percent of the sample members switched occupations at the one-digit level, and 47 percent switched at the two-digit level. The treatment group means show that the highest rate of job-switching at the one-digit level occurred in treatment group 1, where 37 percent of the claimants changed occupation. The lowest rate occurred in treatment group 3, where only 31 percent of the workers switched.

The means for switching indicators at the two-digit occupational level reveal a similar pattern, with 48 percent of the treatment 1 members moving to a different 2-digit occupation, compared with 44 percent of treatment 3 members. However, except for the treatment 1/treatment 3 difference at the one-digit level, none of the differences among treatment groups or the treatment-control differences is statistically significant. Thus, while the data suggest that the bonus offer may have led to less occupational switching than occurred in treatment 1, the evidence on this point is relatively weak. Moreover, there is no indication that any of the treatments affected occupational switching relative to the control group.

4. Summary

In summary, our analysis of the characteristics of the post-UI jobs obtained by claimants shows that none of the treatments had adverse effects on hourly wage rates, hours worked, or occupations. In fact, there is evidence that each of the treatments had modest positive impacts on the hourly wages received by claimants. Thus, any concern that the treatments might have encouraged claimants to accept relatively poor jobs to become reemployed quickly is unfounded.

TABLE VI.15

MEANS OF INDICATORS FOR OCCUPATION-SWITCHING FOLLOWING UI RECEIPT

(Standard Error of Means in Parentheses)

	Full Sample	Treatment Groups			Control Group
		JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus	
Mean Indicator for Switch at One-Digit Level	0.34	0.37 (0.02)	0.33 (0.02)	0.31 (0.02)	0.33 (0.01)
Mean Indicator for Switch at Two-Digit Level	0.47	0.48 (0.02)	0.46 (0.01)	0.44 (0.02)	0.48 (0.02)
Sample Size	4,202	819	1,347	898	1,138

VII. BENEFIT-COST ANALYSIS

In this chapter, we combine estimates of the impacts of the NJUIRDP on earnings and UI benefits that were presented in earlier chapters with estimates of the net costs of the demonstration, so as to assess whether, compared with the existing UI system, the benefits of each treatment exceeded its costs, or vice versa. We examine benefits and costs from several perspectives--those of the major groups affected by these policies (claimants, employers, and the government budget) and the perspective of society as a whole. The purpose of this exercise is to summarize the information learned from the impact evaluation and cost estimation in a way that will help policymakers determine the relative desirability of implementing any of these treatments on an ongoing basis according to the relevant perspectives. Several issues are addressed in our benefit-cost evaluation:

- The costs of providing each of the three treatments on an ongoing basis relative to the costs of providing existing services (these are referred to as net costs)
- The effects of each treatment compared with existing services from the perspectives of society as a whole, claimants, employers, and the government (these are referred to as net effects)--that is, whether benefits outweigh costs or vice versa according to each perspective
- Whether the offer of training and relocation assistance in treatment 2 and/or the offer of the reemployment bonus in treatment 3 generated benefits which exceeded the additional costs of these two treatments once they went beyond the JSA services that comprised treatment 1
- How the costs and benefits of the treatments are allocated between "Department of Labor" programs (i.e., UI, ES, and JTPA programs at the local, state, and federal levels) and the rest of the government sector The implications of this distribution for funding future programs.
- The sensitivity of these conclusions to the assumptions underlying the benefit-cost analysis.

We explored these issues by using a comprehensive benefit-cost analytical framework developed by Mathematica Policy Research staff.[1] Section A discusses the components of this analytical methodology and the benefits and costs that are examined with it. (Appendix E discusses the technical aspects of the methodology and our assumptions in detail.) Section B presents our estimates of the net costs of the three treatments tested in the demonstration, and Section C presents the results of our benefit-cost analysis of the three treatments. Section D provides a summary and the conclusions of our analyses.

---

[1] See Long, Mallar, and Thornton (1981) for one of the initial applications of this framework.

A. METHODOLOGY

The comprehensive accounting framework used to compare the benefits and costs of the three treatments used in the NJUIRDP from the perspectives of society as a whole, UI claimants, employers, and the government encompasses the following steps:

- Defining the various perspectives whereby the benefits and costs to each of the relevant groups can be summed together to represent the benefits and costs to society as a whole
- Determining all potential costs and benefits from each perspective, including those whose value is more intangible and cannot be measured directly
- Assigning a dollar value to each benefit and cost
- Integrating all the benefits and costs from each perspective into a summary table to assess how much each element contributes to the "bottom line"
- Analyzing the sensitivity of the estimates to key assumptions to assess the effects of changing the assumptions on the overall benefit-cost analysis and its conclusions

1. The Various Perspectives

The benefit-cost analysis applied here encompasses a comprehensive accounting of all benefits and costs of the treatments from a number of perspectives. We consider the benefits and costs to the UI claimants themselves, to determine whether the treatments were beneficial to those whom they were designed to serve. We also consider the perspective of the employers who hired the claimants, to examine the net effects of their hiring decisions, and we consider the perspective of the government, to assess the budgetary impacts of each treatment relative to existing programs. We also break the government perspective down into "Labor Department" programs (that is, UI, ES, and JTPA) versus the rest of the network of government programs, to obtain more specific insight into the budgetary implications of these treatments.[2]

Together, these perspectives comprise all of society, and the benefits and costs to each perspective can be summed to derive the net benefits and costs to society as a whole. This social perspective measures whether each treatment increases or reduces the sum total of goods and services available in the economy. Economists refer to a change in goods and services overall as economic efficiency, because it indicates whether society as a whole is producing more with available resources. Transfers between the groups, such as taxes paid by employers or claimants, entail no net change in the resources available to society as a whole, and thus have no impact on the social perspective. Social benefits (or costs) are measured only as gains (or losses) for one group that are not balanced out by equal losses (or gains) for another group. However, transfers between groups have implications for the distribution of resources among groups, which is an important issue for any program.

---

[2] The Labor Department perspective and the rest of the government perspective combined encompass the federal, state, and local levels.

2. Listing Benefits and Costs from Each Perspective

After the relevant perspectives have been defined, the next step in the analysis is to construct a comprehensive list of the expected benefits and costs from each perspective, including those that are difficult to value in dollar terms--for instance, the psychological benefits to claimants of obtaining a job. Although such "intangible" benefits (or costs) are not susceptible to measurement, it is still important that they be assigned to the specific perspective(s), so that policy judgments can be made about how they are likely to affect the measured benefit-cost comparisons.

From the perspective of UI claimants, the key benefit of the demonstration treatments would be an increase in earnings and fringe benefits, due to more rapid reemployment (and/or higher earnings). More rapid reemployment should also be a psychological benefit to claimants, since most people find unemployment stressful. While the reemployment bonus and relocation assistance are direct transfers, the other services provided by the demonstration help claimants only if they increase their earnings. On the other hand, the increased reporting requirements under the demonstration imposed a cost on claimants by reducing their time for leisure or non-market work activities. Other costs to claimants are the loss in some UI benefits from more rapid reemployment, the additional taxes that they are required to pay on their increased earnings and any costs of working (e.g., child care or transportation). On the whole, we expect claimants to have derived net benefits from the demonstration.

Employers benefit from the value of the increased output produced by claimants who are hired more rapidly due to the treatment, but they also incur costs because they must compensate these employees through salaries and fringe benefits (which also include payroll taxes). We make the assumption that the value of the additional output to employers equals the value of the additional compensation by employers, which implies that they will incur no net benefits or costs from these treatments. However, this assumption may understate the benefits derived by employers from a better functioning labor market, which would reduce their recruiting and turnover costs. Alternatively, the treatments might impose a cost on some employers if some temporarily laid-off workers were unavailable for rehire. In addition, both employers and claimants could be affected by any changes in taxes due to an increase (or reduction) in government costs from offering the treatments. However, any such changes would occur only in the long run, and their effect would depend on how the treatments were funded. For this reason, they are not included in this analysis.

The Labor Department perspective includes the perspective of UI and ES, whose operations are funded through the UI payroll tax, and the perspective of the JTPA system, which is funded through general revenues. The costs of providing each of the three treatments in an ongoing program would be incurred by these three agencies. However, they would benefit from their direct share of the tax increases paid by claimants and their employers,[3] and from reductions in UI benefits paid to claimants and in the associated administrative costs of providing those benefits. In addition, their costs would be partially offset by a reduction in the costs of providing existing services. The question of whether the increase in payroll taxes to the Labor Department or the reduction in UI benefits was large enough to offset the net costs of the treatments is one of the key issues in this analysis.[4]

The other sectors of government inevitably derive net benefits from these treatments (assuming the existence of at least some positive impacts on the earnings of claimants) because they receive the portion of claimants' taxes that are not used directly to fund Labor Department programs.[5]

Again, all of these benefits and costs are summed across the perspectives to determine the benefits and costs to society as a whole. On the benefit side, the increased earnings of claimants are considered to represent an increase in total output and thus a net benefit to society. The assumption underlying this approach to valuing output is that the more rapid reemployment of demonstration claimants did not displace the employment of other individuals. This no-displacement assumption seems reasonable given the strength of the New Jersey economy. On the cost side, the net operational costs of offering each treatment represent social resources that could be spent otherwise, and are thus measured as net costs to society.

---

[3] In the analysis, we assigned an increase in UI payroll taxes to the Labor Department share, but we did not assume that the Labor Department would automatically obtain a share of any increase in other taxes.

[4] As noted on the previous page, the reduction in UI benefit receipt might, in the long-run, reduce UI tax rates, thus decreasing payroll taxes. This potential long-run effect is ignored in the analysis.

[5] Other governmental sectors could also benefit if the treatments reduced the receipt of such benefits as Food Stamps or other public assistance. This issue was examined in the analysis, but since no impacts were found we have not included these potential impacts in the benefit-cost framework.

3. Assigning Dollar Values to the Benefits and Costs from Each Perspective

The general approach used to value the benefits and costs of the three treatments is to measure the market value of the resources consumed, saved, or produced due to the treatment, as compared with the existing services available to UI claimants. The market value of these resources was estimated for the time period during which the demonstration operated, which corresponds roughly to fiscal year 1987.[6] Actual market prices were used whenever available, on the assumption that these prices are the best measure of the true costs of these resources. When market prices were not available, the dollar value of resources was estimated. For example, we estimated the value of fringe benefits, taxes, and the administrative costs of government agencies (see Appendix E for details on their estimation).

While it is reasonable to assume that the costs of the demonstration treatments occurred in the year after the initial claim, it is possible that the benefits are realized over a longer period of time. For treatments 1 and 3, the long-term benefits are expected to be small, because they focus on helping claimants become reemployed rather than on improving the level of the claimants' skills. A gain of a few more months of work experience is expected to have only a negligible effect on earnings after those months. However, for treatment 2, longer-term effects are considered to be much more plausible, since this treatment involved the offer of training. Those who took advantage of the training offer may ultimately earn higher wages than they would have earned otherwise, although they may work less in the short run. As discussed in the previous chapter, however, the available data provide no evidence that the impacts of treatment 2 would persist beyond a year after the initial claim filing date. We thus examined impacts on earnings only for the year after that date.

Our estimates of the net costs of providing the demonstration services are based on the estimates of demonstration costs that were prepared for the "Implementation and Process Report." In that report, we attempted to estimate the cost per claimant of offering each of the three treatments as an ongoing program. Costs that were plausibly due to the demonstration and limited-scale nature of the project were subtracted from estimates of the total expenditures of the demonstration.

Our focus in the benefit-cost analysis is a bit different--we wish to measure the net cost of each of the three treatments (again, using only those costs that would be incurred in an ongoing program) relative to the costs of the UI, ES, and JTPA services that are currently used by the target population, since claimants in the demonstration treatments received some services that they would have received in the absence of the demonstration. For example, some claimants who were referred by the demonstration to JTPA services would have gone to JTPA for services on their own. In order to measure the extent to which the costs of the demonstration services were greater than the costs of providing the existing services, we compared the costs of demonstration services with the costs of the services received by the control group.

---

[6] The exact time period over which each benefit and cost is measured is discussed in Appendix E. The differences in the time periods were sufficiently slight that adjustments for inflation were deemed unnecessary.

4. Comparing Benefits and Costs in a Summary Table

All of the data used in the benefit-cost analysis of each treatment are presented in summary tables, including figures for each benefit and cost from each perspective. This enables the reader to evaluate in one glance not just whether the treatment increased the goods and services available to society, but the extent to which it represents a redistribution of resources among groups. The role of an individual benefit or cost in determining whether the treatment is a net benefit or cost from a particular perspective can also easily be assessed--for example, an examination of the table reveals whether savings to the government budget generated by a treatment can be attributed to either low treatment costs or to large reductions in UI collected.

5. Sensitivity Analysis of Key Assumptions

The valuation of benefits and costs inevitably rests on a series of assumptions and a series of estimates which themselves are uncertain. Our goal is to lay out the underlying assumptions as clearly as possible so that it is possible for others to check for themselves the effects of changing the assumption. To that end, we also present some reasonable alternatives to the assumptions which we believe might be subject to change and may have substantial quantitative importance in the evaluation of the benefits and costs of these treatments, and we calculate the benefits and costs of the three treatments under these alternative assumptions. This approach also helps account for the uncertainty of the estimates by providing what we believe are reliable upper and lower bounds on the benefits and costs of each treatment.

B. ESTIMATES OF THE COSTS OF SERVICES TO TREATMENTS AND CONTROLS

In this section, we present our estimates of the per-claimant costs that would be incurred if each of the three demonstration treatments were offered as an ongoing program, and the reductions in the costs of existing ES, UI, and JTPA services that would occur. We then use these estimates to derive the net costs of the three demonstration treatments.

1. Demonstration Costs

Our estimates of per-claimant costs for each of the demonstration treatments are presented in Table VII.1. As designed, the JSA-only treatment (treatment 1) is the least expensive, at $169 per claimant, while the JSA plus training or relocation treatment (treatment 2) is the most expensive, at $491 per claimant; the JSA plus reemployment bonus treatment (treatment 3) falls in between, at $300 per claimant. The additional costs for treatments 2 and 3 are almost entirely due to the payments for training and for the reemployment bonus. In addition, the operational costs of treatment 2 are somewhat higher than the operational costs for the other treatments, since treatment 2 members received services from JTPA staff in addition to most of the same services that other treatment members received from ES and UI staff.

As one would expect, the largest proportion of operational costs are attributable to the cost of local office staff time, since these were the staff who provided services directly to claimants. The proportion of operational costs that represent local office staff time ranged from 68 percent of operational costs for treatment 3 to 76 percent for treatment 2.

The costs of payments to training vendors for treatment group 2 members more than equalled the operational costs of the treatment, since, although these services were used by relatively few treatment group 2 members, they involved substantial expenditures for each person in training.[7] The per-claimant cost of relocation assistance was negligible because only a handful of claimants used these services. The average reemployment bonus received per claimant in treatment 3 was $125, which is 42 percent of the total costs per claimant for treatment 3.

2. Costs of ES and UI Services to Controls

Our estimates of the gross costs of the treatments include the time devoted by UI and ES staff to providing services to claimants in the three treatment groups. Some of the same or similar services would have been provided to some treatment group members in the absence of the demonstration. Thus, it is necessary to estimate the costs that would have been incurred to provide these services to treatment members in the absence of the demonstration to estimate the net costs of the treatments.

For the UI system, gross treatment costs include the time devoted by the UI claims examiners and other UI staff to monitoring compliance with the demonstration's reporting requirements

---

[7] Our estimate of the average cost of classroom training provided by the demonstration is $2,723 per participant, and our estimate of the average cost of on-the-job training is $1,960 per participant (see Table IV.7).

TABLE VII.1

ESTIMATED GROSS COSTS PER CLAIMANT
FOR AN ONGOING PROGRAM

(Dollars)

	Treatment Group		
	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus
Average Treatment Payments			
Classroom training	n.a.	224	n.a.
On-the-job training	n.a.	23	n.a.
Relocation assistance	n.a.	1	n.a.
Out-of-area job search	n.a.	1	n.a.
Transportation allowance	n.a.	1	n.a.
Bonus payments	n.a.	n.a.	125
Total	n.a.	250	125
Average Operational Costs			
Local office labor costs[a]	118	183	118
Central office labor costs[a]	25	29	30
Nonlabor costs plus administrative overhead[b]	26	29	26
Total	169	240	174
TOTAL PER CLAIMANT EXPENDITURES	$169	$491	$300

NOTE: Totals may not add due to rounding.

[a] Labor expenditures include all fringe benefits.

[b] This category includes direct items other than labor, as well as indirect costs.

n.a. = not applicable

and performing eligibility determinations if necessary. In order to estimate the net costs of this activity, we assumed that the cost of all nonseparation, nonmonetary determinations performed for treatment group members was included in demonstration-charged costs. We then estimated the cost of performing nonseparation eligibility determinations for control group members to obtain the net cost of this activity. This control group estimate was derived by using data on nonmonetary determinations presented in Table V.5 (there were an average of .415 determinations per control group member) together with a per-determination-cost figure supplied by NJDOL ($10.86). Thus, the cost of these determinations per control group member was roughly $5.

To obtain net ES costs, we assumed that all ES services provided to treatment group members were paid for by the demonstration. To calculate costs for controls, we used the data on the participation of control group members in ES services presented in Table IV.11, as well as administrative cost estimates for each service provided by NJDOL.[8] Overall, we estimated that Employment Service costs for providing services to the control group were $10 per person.

3. JTPA Service Use

Our approach for estimating the net costs for JTPA services was similar to the approach used for the ES services. We assumed that the JTPA training and operational costs attributed to the demonstration included all JTPA services to treatment 2 members.[9] We then estimated the costs of providing JTPA services to members of the control group and the other two treatment groups, since some JTPA services were received by all of these groups.[10] We estimated these costs based on the data on service use presented in Chapter IV and on cost data for JTPA costs per Title III enrollee.[11]

---

[8] Central ES office staff provided us with statewide estimates of the average amount of time involved in each service, and the appropriate salary rate for the staff providing the service. For services provided in groups, such as job-search workshops and testing, we used the average group size in the demonstration to compute a per-person cost for controls. These costs estimates were adjusted to account for the cost of supervision and non-personal service costs.

[9] Training for treatment 2 members that was funded by JTPA Titles IIa and III and the New Jersey Jobs Program was included in the figures for demonstration costs presented in Section B.1, along with the training funded directly by the demonstration.

[10] It was necessary to estimate JTPA costs for members of treatments 1 and 3 in addition to those for the control group, since the level of services received by these groups differed from the level received by the control group. This difference was due in part to the fact that the demonstration treatments substituted for some JTPA-provided services.

[11] The service-use data (Table IV.11) indicated that 4.8 percent of treatment 1 members, 4.6 percent of treatment 3 members, and 9.1 percent of the control group received some services from JTPA in addition to being certified as eligible. For the cost data, we used JTPA Title III average costs per enrollment in Program Year 1986 (July 1986 to June 1987) in each of the SDAs involved in the

Our estimates indicated that JTPA services cost about $99 per claimant in the control group and about $100 and $91 per claimant in treatments 1 and 3, respectively.

4. Net Costs of the Three Treatments Compared with Existing Services

In Table VII.2, we combine all of the cost data presented thus far in order to derive our estimates of the per-claimant net costs of each treatment relative to existing services. In keeping with the demonstration cost estimates presented in Table VII.1, our net cost estimates indicate that treatment 1 was least expensive (at $155 per claimant), treatment 2 was most expensive (at $377 per claimant), and treatment 3 was intermediate in cost (at $277 per claimant). These estimates of the net increases in the costs of providing each of the treatments in an ongoing program are smaller than the gross costs presented in Table VII.1. In particular, 8 percent of treatment 1 gross costs, 23 percent of treatment 2 gross costs, and 8 percent of treatment 3 gross costs were offset because some services were already provided to claimants.

C. BENEFITS AND COSTS FROM ALTERNATIVE PERSPECTIVES

In this section, we provide a set of benchmark estimates of the net benefits and costs of the three treatments relative to the existing services available to UI recipients. We also present the results of a number of sensitivity tests, which allow us to determine the extent to which our results will change if we change particular assumptions.

From the social perspective, we find that the benefits of treatments 1 and 3 substantially exceeded their costs when compared with existing services, while the benefits of treatment 2 only slightly exceeded its costs. All three treatments led to an increase in earnings, which provided net benefits to claimants. Treatments 1 and 3 created savings that exceeded costs from the perspective of the government sector as a whole, but both involved modest net costs to the Labor Department perspective. The rest of government benefited from the increased taxes paid by claimants, primarily on their increased earnings. In general, the results for treatments 1 and 3 were quite similar, while treatment 2 was expensive relative to the other treatments from all perspectives.

---

demonstration. Title III costs were used because the population eligible for Title III is more similar to those eligible for the demonstration than is the overall JTPA population. In using these cost data, we adjusted for the fact that individuals in treatments 1 and 3 who received JTPA services were more likely to receive training than were those in the control group.

TABLE VII.2

ESTIMATED NET COSTS PER CLAIMANT FOR AN ONGOING PROGRAM

(Dollars)

	JSA Only	JSA Plus Training/ Relocation	JSA Plus Reemployment Bonus
Demonstration Costs			
Classroom training	n.a.	224	n.a.
On-the-job training	n.a.	23	n.a.
Relocation assistance[a]	n.a.	3	n.a.
Reemployment bonuses	n.a.	n.a.	125
Local office labor costs[b]	118	183	118
Central office labor costs[b]	25	29	30
Nonlabor costs plus administrative overhead[c]	26	29	26
Total	169	491	300
Nondemonstration JTPA Costs	100	n.a.	91
Offsetting Costs of Existing Services			
ES costs	-10	-10	-10
JTPA costs	-99	-99	-99
UI costs	-5	-5	-5
Total	-114	-114	-114
Net Costs per Claimant	155	377	277

NOTE: Totals may not add due to rounding.

[a] Relocation assistance includes out-of-area job search and transportation allowances.

[b] Labor expenditures include all fringe benefits,

[c] This category includes direct items other than labor, as well as indirect costs.

Our sensitivity tests are intended to show the range of benefit-cost estimates that arise from using alternative assumptions that may be better-suited to some policymakers. The estimated net benefits of all three treatments from the perspective of claimants would be substantially smaller but still positive if we relied on the wage-records data to derive earnings impact estimates. This change would yield net costs to government while the costs from the Labor Department perspective would be roughly the same. Our cost estimates, we believe, are conservative, but we show that a number of seemingly large changes in our assumptions about costs reduce the total costs of the treatments by only modest amounts.

1. Benefit-Cost Analysis of Treatment 1 versus Existing Services

Table VII.3 presents benchmark estimates of the net benefits (or costs) of treatment 1 (intensive job-search assistance) relative to existing services. In general, treatment 1 led to a substantial increase in output and earnings and a moderate reduction in UI benefits collected. As designed, the costs of the treatment were relatively low. We estimate that the benefits of the treatment outweighed the costs from the perspectives of claimants, the government, and society as a whole. The net gain to the government sector, however, was derived from a small net loss to the Labor Department sector and a larger gain to the rest of the government. That is, the reduction in UI payments was insufficient to pay for the cost of the increased services.

More specifically, we estimate that members of treatment 1 increased their earnings by an average of $608 relative to members of the control group, and we impute another $128 in additional fringe benefits to reach a total increase of $736 in compensation. Much of this increase in compensation benefited the claimants, but treatment costs were modest enough, and enough of the increased earnings were returned to the government sector via increased taxes and reduced UI benefits that the government sector realized a net gain of $88 per claimant.[12] When we specifically examine the UI, ES, and JTPA programs which comprise the Labor Department sector, however, we find that they incurred a (modest) net loss of $61 per claimant.

Society as a whole gained from the increased output and reduced expenditures on government services. The net social gain, which can be thought of as an indicator of the efficiency of the treatment, is estimated to be $581 per claimant.

---

[12] There were no observed impacts of the treatment on participation in income support programs other than UI which is why no benefits or costs are reported for these programs. For the UI program there was a reduction in benefits paid and a corresponding small reduction ($1) in the administrative costs of processing claims.

TABLE VII.3

BENEFIT-COST COMPARISON OF THE JSA-ONLY TREATMENT WITH EXISTING SERVICES

(Dollars per Claimant)

				Government		
Benefits and Costs	Society	Employers	Claimants	Labor Dept.	Other Government	Government Total
Market Output and Wages						
Increased output	736	736	0	0	0	0
Wages and fringe benefits	0	-736	736	0	0	0
Tax Payments						
Claimants' taxes	0	0	-156	7	149	156
Income Support Payments						
UI payments	0	0	-87	87	0	87
Other programs	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI payment administration	1	0	0	1	0	1
Administration of other programs	0	0	0	0	0	0
Demonstration Costs						
Classroom training costs	0	0	0	0	0	0
OJT costs	0	0	0	0	0	0
Relocation assistance	0	0	0	0	0	0
Reemployment bonuses	0	0	0	0	0	0
Local office labor costs	-118	0	0	-118	0	-118
Central office labor costs	-25	0	0	-25	0	-25
Other costs (direct and indirect)	-26	0	0	-26	0	-26
Offsetting Costs of Existing Services						
ES costs	10	0	0	10	0	10
JTPA costs	-1	0	0	-1	0	-1
UI costs	5	0	0	5	0	5
Sum of Measured Benefits and Costs	581	0	493	-61	149	88
Nonmonetary Factors						
Psychological benefits of earlier reemployment	+		+			
Burden of reporting requirements, reduced leisure time, costs of work	-		-			

NOTE: Totals may not add due to rounding.

2. Benefit-Cost Analysis of Treatment 2 versus Existing Services

The benefit-cost comparison between treatment 2 and existing services is presented in Table VII.4. Overall, treatment 2 provided net benefits to claimants and net costs to the government sector, while society as a whole roughly broke even.

Our estimates suggest that the earnings of treatment 2 members were an average of $345 higher than those of control group members, and we impute an additional $72 of increased fringe benefits. These increased earnings and fringe benefits represent a benefit to claimants on average, which was partially offset by an increase in taxes of $81 per person, and a reduction in UI benefits also of $81 per person. The estimated net benefit per claimant of treatment 2 (compared with existing programs) was $258.

The very substantial costs of providing training, even to the minority of treatment 2 members who pursued this option, meant that the government sector incurred net costs for treatment 2. Treatment 2 members were three to four times as likely to pursue training as were control group members. In addition, they received the same set of initial job-search services from the ES as did those in treatment 1. The increased taxes and reduced UI benefits generated by the increased employment of persons in treatment 2 substantially offset the costs of the JSA services, but did not begin to cover the costs of the training itself. The net government loss of $214 per claimant can be broken down into a $291 loss for Labor Department programs, and a $78 gain for the rest of the government.

From the perspective of society as a whole, the choice between treatment 2 and existing services appears fairly even, with a slight net benefit of $44 per person in favor of the treatment.

Compared with treatment 1, treatment 2 benefits were lower relative to costs from all perspectives. In particular, treatment 2 members realized lower earnings increases on average, but the costs of the treatment were much higher. It seems that the most plausible interpretation is that treatment 2 members who entered training (or hoped to enter training) deferred reentering the labor market, and did not increase their earnings capacity sufficiently to compensate for the fewer number of weeks that they worked in the year after the claim filing date. The possibility that the impacts of treatment 2 on earnings are underestimated because we use only the year after the claim filing date is explored below.

TABLE VII.4

BENEFIT-COST COMPARISON OF THE JSA TRAINING OR RELOCATION ASSISTANCE TREATMENT WITH EXISTING SERVICES

(Dollars per Claimant)

				Government		
Benefits and Costs	Society	Employers	Claimants	Labor Dept.	Other Government	Government Total
Market Output and Wages						
Increased output	417	417	0	0	0	0
Wages and fringe benefits	0	-417	417	0	0	0
Tax Payments						
Claimants' taxes	0	0	-81	4	78	81
Income Support Payments						
UI payments	0	0	-81	81	0	81
Other programs	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI payment administration	1	0	0	1	0	1
Administration of other programs	0	0	0	0	0	0
Demonstration Costs						
Classroom training costs	-224	0	0	-224	0	-224
OJT costs	-23	0	0	-23	0	-23
Relocation assistance	-3	0	0	-3	0	-3
Reemployment bonuses	0	0	0	0	0	0
Local office labor costs	-183	0	0	-183	0	-183
Central office labor costs	-29	0	0	-29	-	-29
Other costs (direct and indirect)	-29	0	0	-29	0	-29
Offsetting Costs of Existing Services						
ES costs	10	0	0	10	0	10
JTPA costs	99	0	0	99	0	99
UI costs	5	0	0	5	0	5
Sum of Measured Benefits and Costs	44	0	258	-291	78	-214
Nonmonetary Factors						
Psychological benefits of earlier reemployment	+		+			
Burden of reporting requirements, reduced leisure time, costs of work	-		-			

NOTE: Totals may not add due to rounding.

3. Benefit-Cost Analysis of Treatment 3 versus Existing Services

The results of our benefit-cost analysis of treatment 3--job search assistance plus the reemployment bonus--are presented in Table VII.5. Treatment 3 members experienced similar earnings gains to claimants in treatment 1 and larger UI benefit reductions than did the other two treatment groups, while the costs of the treatment fell in between the costs of treatments 1 and 2. On balance, there was a substantial net gain to society of $565 per claimant relative to existing services.

Claimants experienced a net benefit of $510 on average, comprising a $591 increase in earnings and a $124 increase in fringe benefits, balanced by a $170 reduction in UI benefits and a $161 increase in taxes. The government benefited overall, but Labor Department programs suffered a net loss of $99 per claimant, because the added cost of the bonus payments was larger than the savings in benefits and the additional taxes paid. The rest of the government sector experienced a net gain of $154 from an increase in taxes.

Overall, the findings for treatment 3 were quite similar to those for treatment 1. The earnings gains experienced by claimants were similar, and while the bonus payments represented a cost to the government sector and a gain to claimants, this cost (and gain) was partially offset by the larger reduction in UI payments relative to treatment 1. However, because this reduction in UI payments plus the increase in taxes relative to treatment 1 did not completely offset the cost of the bonus, this treatment, as implemented, was $33 more expensive per claimant for the government sector than was treatment 1.

4. The Sensitivity of the Estimates to Alternative Assumptions

The estimates presented in the preceding sections are based on a number of assumptions which could be questioned. The estimates of UI and earnings impacts that are used are also subject to some uncertainty. Thus, it is useful to consider the sensitivity of the estimates to changes in the assumptions or estimates that are used to generate the measured benefits and costs. We first consider the implications for the analysis had we decided to use earnings impact estimates based on the wage-records data rather than estimates based on the interview data. The implications of the possibility that the impacts of treatment 2 may persist beyond the time period that is observed are also explored.

TABLE VII.5

BENEFIT-COST COMPARISON OF THE JSA PLUS REEMPLOYMENT BONUS TREATMENT WITH EXISTING SERVICES

(Dollars per Claimant)

				Government		
Benefits and Costs	Society	Employers	Claimants	Labor Dept.	Other Government	Government Total
Market Output and Wages						
Increased output	715	715	0	0	0	0
Wages and fringe benefits	0	-715	715	0	0	0
Tax Payments						
Claimants' taxes	0	0	-161	7	154	161
Income Support Payments						
UI payments	0	0	-170	170	0	170
Other programs	0	0	0	0	0	0
Administrative Costs of Income Support Programs						
UI payment administration	1	0	0	1	0	1
Administration of other programs	0	0	0	0	0	0
Demonstration Costs						
Classroom training costs	0	0	0	0	0	0
OJT costs	0	0	0	0	0	0
Relocation assistance	0	0	0	0	0	0
Reemployment bonuses	0	0	125	-125	0	-125
Local office labor costs	-118	0	0	-118	0	-118
Central office labor costs	-30	0	0	-30	0	-30
Other costs (direct and indirect)	-26	0	0	-26	0	-26
Offsetting Costs of Existing Services						
ES costs	10	0	0	10	0	10
JTPA costs	8	0	0	8	0	8
UI costs	5	0	0	5	0	5
Sum of Measured Benefits and Costs	565	0	510	-99	154	55
Nonmonetary Factors						
Psychological benefits of earlier reemployment	+		+			
Burden of reporting requirements, reduced leisure time, costs of work	-		-			

NOTE: Totals may not add due to rounding.

A second set of sensitivity tests explore the possibility that we have overstated the costs of providing the demonstration treatments as ongoing programs. We believe that the benchmark estimates presented above represent upper-bound estimates of the costs of providing the three treatments, since these estimates do not take into account the fact that the demonstration may have operated less efficiently than would be the case in an ongoing program. Thus, we present some alternative cost estimates which are based on fairly arbitrary assumptions about the degree to which efficiency gains might be realized. For this reason, these alternative estimates should be viewed as a set of lower-bound estimates. However, it should be noted that while costs might be lower in an ongoing program relative to the actual costs in NJUIRDP a reduction in costs and staff effort might also affect the outcomes of the program. Thus, these lower-bound estimates should be viewed with caution.

a. Alternative Estimates of Earnings Impacts

We chose to use what we believe are the best estimates of the impacts of the demonstration as our estimates of the benefits of each treatment. In particular, we have used estimates of earnings impacts drawn from the follow-up interview data. The wage-records data provide an alternative set of estimates of earnings impacts, which in all cases are lower than those estimated from the interview data. As discussed in Chapter VI, the wage-records data understate earnings impacts because (1) the data do not include the earnings of the self-employed and those who find new employment outside New Jersey, and (2) the data cover the first calendar quarter after the claim date, and thus miss treatment-control differences in the first month or two after the claim filing date for much of the sample. Other results suggest that this is the point at which much of the impacts of the treatments occur.

Our first sensitivity tests calculated the benefits and costs of the three treatments using the estimates of earnings impacts from the wage-records data. The results from these tests are presented in Tables VII.6-VII.8. For all three treatments, using earnings estimates based on the wage records greatly reduces the estimated benefits to claimants, government, and society as a whole relative to our benchmark estimates. The net benefits and costs from the Labor Department perspective scarcely change, because we are not changing the impacts of the treatment on UI benefits. For treatments 1 and 3, however, the net benefits to claimants and society as a whole remain positive, while remaining more expensive to the government. If the wage-records data were an accurate measure of earning impacts for treatment 2, the treatment would on balance be expensive from all perspectives, even for claimants (since, as before, their UI benefits decline).

TABLE VII.6

SUM OF BENEFITS AND COSTS OF THE JSA-ONLY TREATMENT
UNDER ALTERNATIVE ASSUMPTIONS

(Dollars Per Claimant)

			Government		
	Society	Claimants	Labor Dept.	Other Government	Government Total
Benchmark Estimates	581	493	-61	149	88
Using Earnings Impacts from Wage-Records Data	41	83	-66	25	-41
With a 50 Percent Reduction in Central Office Labor Costs	594	493	-48	149	101
With Adjustment for Scale	587	493	-55	149	94
With a 50 Percent Reduction in Central Office Labor Costs and Adjustment for Scale	600	493	-42	149	107

NOTE: Totals may not add due to rounding.

TABLE VII.7

SUM OF BENEFITS AND COSTS OF THE JSA PLUS TRAINING OR RELOCATION ASSISTANCE TREATMENT UNDER ALTERNATIVE ASSUMPTIONS

(Dollars Per Claimant)

			Government		
	Society	Claimants	Labor Dept.	Other Government	Government Total
Benchmark Estimates	44	258	-291	78	-214
Using Earnings Impacts from Wage-Records Data	-286	7	-295	2	-293
With a 50 Percent Reduction in Central Office Labor Costs	59	258	-277	78	-199
With a 50 Percent Reduction in Central Office and JTPA Local Office Costs	91	258	-245	78	-167
With Adjustment for Scale	52	258	-284	78	-206
Using Demonstration Measure of JTPA Participation	91	258	-245	78	-167
With a 50 Percent Reduction in JTPA and Central Office Costs, Adjustment for Scale, and Demonstration Measure of JTPA Participation	145	258	-191	78	-113

NOTE: Totals may not add due to rounding.

TABLE VII.8

SUM OF BENEFITS AND COSTS OF THE JSA PLUS REEMPLOYMENT BONUS TREATMENT UNDER ALTERNATIVE ASSUMPTIONS

(Dollars Per Claimant)

			Government		
	Society	Claimants	Labor Dept.	Other Government	Government Total
Benchmark Estimates	565	510	-99	154	55
Using Earnings Impacts from Wage-Records Data	240	263	-102	79	-23
With a 50 Percent Reduction in Central Office Labor Costs	580	510	-84	154	70
With Adjustment for Scale	571	510	-92	154	62
With a 50 Percent Reduction in Central Office Labor Costs and Adjustment for Scale	586	510	-77	154	77

NOTE: Totals may not add due to rounding.

Another area of possible uncertainty is the persistence of earnings impacts into the future. Based on the interview sample for whom we have fifth- and sixth-quarter earnings, we do not find significant earnings impacts after the first year for any of the treatment groups. As noted earlier, this finding makes sense for treatments 1 and 3, but is more surprising for persons in treatment 2, since some of them participated in training for much of the year after the initial UI claim filing date, and thus, although we would expect any earnings impacts for treatment 2 members to appear later than those for the other treatment members, we would also expect them to persist for a longer period of time. If treatment 2 earnings impacts persisted for three years (at the level measured with the interview data), the treatment would generate net benefits that would exceed those of existing services from the perspectives of claimants and society as a whole. However, this level of persistence would not be enough for the government sector to realize benefits relative to existing services.

b. Alternative Estimates of Net Costs

In our estimates of treatment costs, we have attempted to be conservative in our assumptions, so that when benefits exceed costs under these assumptions we can be fairly confident that benefits do indeed exceed costs. Our cost estimates should thus be considered as an upper bound on true costs.

One reason for believing that the true costs of an ongoing treatment may be lower than our benchmark estimates is that staff costs might be lower in an ongoing program, even though we excluded costs for staff whose functions were clearly associated with the research and planning aspects of the demonstration. Staff costs may be overstated for three major reasons: (1) less central office staff time may be required for effecting coordination among divisions in an ongoing program, (2) JTPA staff may devote less time, since the proportion of those offered training who expressed interest was less than anticipated, and (3) an ongoing program might operate at a more efficient scale, since the demonstration used roughly the same number of staff in each office, even though several served smaller caseloads. In order to examine these issues, we repeat here three sensitivity analyses discussed in more detail in the "Implementation and Process Report," and consider their results for the benefit-cost analysis, rather than the analysis of costs alone. These three tests are to (1) recalculate costs under the assumption that central office labor costs fall by 50 percent, (2) recalculate costs under the assumption that JTPA local office costs fall by 50 percent, and central office staff costs fall by 50 percent, and (3) recalculate local office labor costs using data only from the 6 sites which served the largest numbers of treatment-group members, to adjust for the fact that some sites operated at a smaller scale than did others, with the same level of staff. These three changes both by themselves and together provide us with plausible lower bounds for the operational costs of the treatments.

In addition, we may have overestimated training costs. A number of treatment 2 members are recorded as having received classroom training or OJT in the JTPA data system, but not in the records maintained by the JTPA counselors who were working on the demonstration. Thus, we do not know the costs of their training. We assumed that their training cost was the same, on average, as the training of the group of training participants for which we have cost data. Investigations by NJDOL staff, however, suggest that the training received by these members of treatment 2 may have been less expensive, either because they did not complete the program or because they were referred to a less expensive type of training program than were other trainees (sometimes funded under JTPA Title IIA, rather than under Title III). Rather than attempting to track down training expenditures for these cases, we simply examined training expenditures solely for those training participants who were tracked by the demonstration, a figure which we know provides a lower-bound estimate of expenditures on training.

The results of these tests are also presented in Tables VII.6 through VII.8. In general, they lead to slight reductions in costs (or increases in benefits) for the Labor Department sector, the government as a whole, and society. The two tests that applied to treatment 2 alone--the 50 percent reduction in JTPA costs and central office costs--and using the lower measure of participation in training reduced estimated costs by the most substantial amounts ($46, or about 10 percent of demonstration costs, for each change). Moreover, the combination of these changes with the other cost changes would have increased the net social gain for treatment 2 from $44 to $145. However, this increase in net social gain does not change our overall conclusions about our comparisons of the three treatments.

D. SUMMARY AND DISCUSSION

The results of our benefit-cost analysis suggest that all three treatments offer net benefits to society as a whole and to claimants when compared with existing services. Treatments 1 and 3 also lead to net gains to the government sector as a whole, but not to the Labor Department agencies which actually offered the services. Treatment 2 is expensive for the government sector.

When we compare treatment 2 with treatment 1, we find that treatment 2 costs were higher (or benefits were lower) than those for treatment 1 from all viewpoints, since the costs of the service component of treatment 2 were higher and its earnings gains were substantially lower. Treatment 2 would look better if earnings gains could be sustained for at least three years, but the limited data we have on earnings in the fifth and sixth quarters after the claim filing date do not show any earnings differences between treatment 2 members and control group members.

The benefits and costs of treatment 3 appear to be similar to those of treatment 1 from all perspectives, although it generated slightly higher costs from the government perspective. This occurred because the greater reductions in UI benefits in treatment 3 relative to treatment 1 did not fully balance the cost of the bonus from the government perspective.

Our sensitivity analyses showed that our decision to rely on the interview data for measures of earnings impacts rather than on the wage- records data was important--but that treatments 1 and 3 would have positive net benefits from the social perspective even with the wage-records data. In addition, these analyses showed that the differences between our upper- and lower-bound estimates of treatment costs do not lead to major changes in the "bottom-line" estimates of the net benefits (or costs) of the three treatments.

These findings suggest that it would not be possible for any of the treatments to be funded solely from the savings in UI benefits and increased UI taxes (although treatment 1 comes closest). Treatments 1 and 3 could be funded from the reduction in UI benefits and the increase in tax revenues overall, if it were possible to reallocate tax money that would currently go to other sectors of the government to cover the additional costs of these programs. Treatment 2 would need to be funded either through a reduction in spending on other programs or an increase in taxes, since it appears to create net costs to the government as a whole.

VIII. IMPLICATIONS OF THE FINDINGS FROM THE DEMONSTRATION EVALUATION

The findings presented in the previous chapters indicate that the demonstration was generally successful at identifying and offering services to a group of claimants who, in the absence of the demonstration, would have experienced difficulties in becoming reemployed. The demonstration treatments were also successful at providing services to these individuals early in their unemployment spells and at increasing their level of service use relative to what would have been the case with the existing service system. Each of the treatments also appears to have hastened the reemployment of claimants, and thus, both earnings rose and the amount of UI benefits collected declined relative to the levels observed for claimants who were not offered the treatments. Finally, relative to existing services, each of the treatments appears to have generated net benefits to society as a whole and to claimants. Two of the treatments were also beneficial from the standpoint of the government as a whole, although none of them generated net benefits from the narrower perspective of the Labor Department agencies that operated the demonstration.

These overall, positive findings suggest that the demonstration treatments represent potentially useful reemployment policies that could be directed toward UI claimants. However, several issues associated with the treatments bear further discussion: (1) the targeting of demonstration services, (2) the use of participation requirements, and (3) the selection of services.

A. TARGETING SERVICES

An important issue for any reemployment strategy is deciding who should be provided with services. The eligibility definition used in the New Jersey demonstration attempted to target services toward displaced workers who would experience reemployment difficulties. Our evaluation indicated that, although some individuals who were made eligible for services faced good reemployment prospects and some who were not eligible faced poor reemployment prospects, the particular eligibility definition chosen for the demonstration was generally successful at achieving its target objective. The reemployment experience of members of the demonstration-eligible control group were, on average, considerably worse than those of individuals who were not eligible for the demonstration.

However, our evaluation also showed that, among demonstration- eligibles, the treatments, particularly the job-search-assistance-only treatment, were most successful at promoting the reemployment of individuals who had marketable skills, such as clerical and other white-collar workers.[1] The treatments were less successful for individuals who faced hard-core, structural unemployment problems, such as blue-collar workers, workers from durable-goods manufacturing industries, and permanently separated workers. That is, the displaced workers with more severe reemployment problems may have been affected less by the treatments than were other workers who faced relatively more favorable reemployment prospects.

This finding is perhaps not surprising, since each of the treatments emphasized self-directed job search by providing an intensive set of job-search services (including a job-search workshop) during the initial service period. These services are designed for individuals who have marketable skills, and it appears that this group was the one that found jobs more readily than they would have otherwise.

This finding also suggests that the treatments, particularly the initial mandatory job-search assistance services, are appropriate and cost-effective for a broad-range of UI claimants who meet reasonable operational definitions of displacement, but that longer-run, more intensive services are needed for displaced individuals who face major structural dislocations. The demonstration did offer occupational training in the second treatment (see further below), but additional services may be needed. For example, the high rate of excusals from testing and the job-search workshop for language and literacy reasons suggests that referrals to English as a Second Language or remedial education services may be needed for some individuals.

B. USING PARTICIPATION REQUIREMENTS

The job-search assistance components of the treatments instituted in New Jersey included a UI requirement that claimants report for services. Failure to report for services could have led to the denial of benefits. Our analysis of this requirement and of the compliance process associated with it found evidence that this requirement was enforced reasonably well. Most individuals who did not report for the initial job-search services were subject to a benefit eligibility review or had some clear reason for nonreporting (e.g., they stopped claiming benefits, or they moved from a demonstration area to another area). Moreover, the rate at which the eligibility of claimants was questioned and denied was greater for members of the treatment groups than for members of the control group.

---

[1] A New Jersey Department of Labor demonstration of job-search assistance run prior to the NJUIRDP had success with a similar group of claimants. That demonstration focused intensive job-search assistance on claimants whose occupations were considered to be in demand, and the evaluation found that this treatment led to a substantial reduction in the duration of UI benefits (2.6 weeks) and in total benefits paid ($340) over the benefit year (NJDOL, 1987).

Given these findings it is important to ask if a program in which participation in the services was voluntary would have produced the same results. The design of the demonstration does not enable us to address this question directly, since all of the treatments encompassed the same structure of mandatory components. However, some indirect evidence is available that the mandatory nature of the treatments was important. Participation levels in the initial services were higher than one would expect in a voluntary program. Some of the largest impacts observed for subgroups occurred for individuals who expected to be recalled, a group that would not have been likely to respond to a voluntary program. And much of the impact of the demonstration appeared to occur very quickly. For example, the weekly exit rate from UI was higher for treatment members than for control group members beginning with the fifth week of unemployment when claimants were initially called-in for services. We conclude that the reporting and compliance processes were probably important contributing factors to the impacts of the treatments.

C. SELECTING SERVICES

The demonstration tested three packages of services: (1) job-search assistance (JSA) only, (2) job-search assistance plus training or relocation assistance, and (3) job-search assistance plus a reemployment bonus. Several observations can be made about the relative effectiveness of these three treatments.

Our findings generally found that the job-search assistance component of the treatments was successful at promoting the reemployment of claimants. Our examination of impacts on UI receipt showed that the JSA-only treatment reduced the amount of UI received. It also showed that the rate of exit from the UI system increased early in individuals' claim spells, a time period in which intensive job-search assistance was provided. Similarly, we found positive impacts on employment and earnings early in individuals' claim spells. The benefit-cost analysis also indicated that the JSA-only treatment generated a net social benefit. Our findings indicated that the addition of the training or relocation offer to the basic job-search assistance services did not lead to larger short-run impacts. In fact, as could be expected, the impacts were slightly smaller, because individuals who entered training continued to collect UI and delayed their return to employment. Moreover, since the cost of the training itself is high, the training treatment was expensive relative to the other treatments, even though only a small percentage of individuals received training. However, these findings should not be viewed as indicating that training should not be offered. Training could have longer-run impacts which we were not able to measure in this study, and it may potentially be valuable for the individuals who do not have marketable skills on whom the treatments had little impact.

Our findings about the reemployment bonus offer showed that the amount of UI benefits received by claimants who were offered the bonus was significantly less than the amount received by claimants who were not. Differences in employment and earnings between those who were offered the bonus and those who were not were positive for the first two quarters after the claim filing date although these differences did not persist into later quarters. Only the first quarter earnings impact was statistically significant. These findings concerning the impact of the bonus offer on UI receipt and earnings suggest that the bonus offer helped hasten the reemployment of claimants. However, our benefit-cost analysis indicated that the additional UI savings generated by the bonus offer did not offset the cost of the bonus itself, and that the gains in earnings were not sufficiently greater than those obtained from the JSA-only treatment that they make a difference in the benefit-cost comparisons. Overall, the job-search assistance-only and job-search assistance plus reemployment bonus treatments had very similar benefit-cost outcomes from all perspectives. Thus, the New Jersey demonstration evaluation results suggest that a reemployment bonus offer does not improve labor-market outcomes sufficiently to make the combination of mandatory job-search assistance plus the bonus offer a more successful treatment than mandatory job-search assistance alone. An unanswered question is how a bonus offer by itself would compare with job-search assistance alone, particularly if job-search assistance contains mandatory elements, as was the case in New Jersey. Two other demonstrations, in Pennsylvania and Washington, are exploring a wide-range of reemployment bonus schemes.

REFERENCES

Bloom, H.S. and J. Kulik. "Evaluation of the Worker Adjustment Demonstration: Final Report." Cambridge, MA: Abt Associates, July 1986.

Congressional Budget Office. "Promoting Employment and Maintaining Income with Unemployment Insurance." Washington. D.C.: Congressional Budget Office, March 1985.

Corson, W. and S. Dunstan. "Implementation and Process Report." Part 2 in The New Jersey Unemployment Insurance Reemployment Demonstration Project: Final Evaluation Report. Princeton, NJ: Mathematica Policy Research, 1989.

Corson, W., S. Kerachsky, and E. Eliason-Kisker. "Work Search Among Unemployment Insurance Claimants: An Investigation of Some Effects of State Rules and Enforcement." UI Occasional Paper 88-1. Washington, D.C.: U.S. Department of Labor, Employment and Training Administration, 1988.

Corson, W., D. Long, and W. Nichoison. "Evaluation of the Charleston Claimant Placement and Work Test Demonstration." UI Occasional Paper 85-2. Washington, D.C.: U.S. Department of Labor, Employment and Training Administration, 1985.

Corson, W., D. Long, and R. Maynard. "An Impact Evaluation of the Buffalo Dislocated Worker Demonstration Project." Princeton, NJ: Mathematica Policy Research, March 1985.

Corson, W. and W. Nicholson. "An Analysis of UI Recipients' Unemployment Spells." UI Occasional Paper 83-1. Washington, D.C.: U.S. Department of Labor, Employment and Training Administration, 1985.

Corson, W. et al. "The Design of the New Jersey Unemployment Insurance Reemployment Demonstration Project." Princeton, NJ: Mathematica Policy Research, 1986.

Crosslin, R., J. Hanna, and D. Stevens. "Using Unemployment Insurance Data to Predict Economic Hardship from Displacement." In Displaced Workers: Implications for Educational and Training Institutions, edited by K. Hollenbeck, F.C. Pratzner, and H. Rosen. Columbus, OH: The National Center for Research in Vocational Education, The Ohio State University, 1984.

Flaim, P.O. and E. Sehgal. "Displaced Workers of 1979-83: How Well Have They Fared?" Monthly Labor Review, vol. 108, no. 6, June 1985, pp. 3-16.

Gordon, N. "Job Displacement and Employment Security: Reactor Comments." In Displaced Workers: Implications for Educational and Training Institutions, edited by K. Hollenbeck, F.C. Pratzner, and H. Rosen. Columbus, OH: The National Center for Research in Vocational Education, The Ohio State University, 1984.

Gustman, A. "Analyzing the Relationship of Unemployment Insurance to Unemployment." Working Paper no. 512. Cambridge, MA: National Bureau of Economic Research, July 1980.

Hamermesh, D. Jobless Pay and the Economy. Baltimore, MD: Johns Hopkins University Press, 1977.

Kalbfleisch, J. and R. Prentice. The Statistical Analysis of Failure Time Data. New York, NY: Wiley Press, 1980.

Kiefer, N. "Economic Duration Data and Hazard Functions." Journal of Economic Literature, vol. 24, no. 2, June 1988, pp. 646-679.

Kulik, J., D.A. Smith, and E. Stromsdorfer. "The Downriver Community Conference Economic Readjustment Program: Final Evaluation Report." Cambridge, MA: Abt Associates, Inc., May 18, 1984.

Long, D., C. Mallar, and C. Thornton. "Evaluating the Benefits and Costs of the Job Corps." Journal of Policy Analysis and Management, vol. 1, no. 1, 1981, pp. 55-76.

N.J. Department of Labor. "Evaluation of the Perceivable Demand List: Pilot Project." Trenton, N.J.: N.J. Department of Labor, Division of Planning and Research, Office of Program Research, October 1987.

Richardson, P. et al. "Referral of Long-Term Unemployment Insurance Claimants to Reemployment Services." Washington, D.C.: Macro Systems, Inc., July 1988.

U.S. Department of Labor. "Alternative Uses of Unemployment Insurance." UI Occasional Paper 86-1. Washington, D.C.: U.S. Department of Labor, Employment and Training Administration, 1986.

Vroman, W. "Innovative Developments in Unemployment Insurance." Washington, D.C.: National Commission for Employment Policy, Research Report Series, RR-85-02, February 1985.

Westat. "Job Search and Relocation Assistance: Final Report." Rockville, MD: Westat, 1981.

Woodbury, S.A. and R.G. Spiegelman. "Bonuses to Workers and Employers to Reduce Unemployment: Randomized Trials in Illinois." The American Economic Review, vol. 77, no. 4, September 1987, pp. 513-530.

APPENDIX A

DATA SOURCES

This appendix briefly describes the six main data sources used for the impact analysis of the New Jersey UI Reemployment Demonstration Project. These are: (1) the Participant Tracking System, (2) UI administrative records, (3) quarterly wage records, (4) Employment Service administrative records, (5) JTPA administrative records, and (6) the follow-up interview. Data from the first five sources are available for all claimants in the sample while the interview data are available for a subsample (see Chapter II for a description of the two samples).

A. THE PARTICIPANT TRACKING SYSTEM

A Participant Tracking System (PTS) was developed for the sample selection and assignment task and for use in the provision of the demonstration treatments. This system included (1) summary data from the UI data system, (2) data on claimant characteristics collected on a short questionnaire administered to all claimants during their Benefit Rights Interview, (3) data from the sample selection process, and (4) data on claimant's participation in the demonstration services. All four types of data were used in the analysis.

The summary data from the UI system included data on claimants' age, race, and sex together with data on base period earnings, the initial claim, the UI entitlement, nonmonetary determinations, and the amount of UI collected. The claimant questionnaire included variables used for eligibility determination such as recall status, tenure on the pre-UI job, and use of a union hiring hall. The date of selection into the sample and the treatment-control assignment came from the sample selection process.

Finally, the majority of the PTS data items provided information on claimants' participation in the demonstration including scheduled dates for services and the services received by claimants. Data for both the initial job-search assistance services and subsequent services such as training, relocation assistance and the reemployment bonus were included.

B. UI ADMINISTRATIVE RECORDS

In addition to the UI data which were added to the PTS and which are described above, UI records for each weekly claim were collected. These records included the date of claim, the date of any payment, the amount of payment, any deductions for earnings, pensions, or other reasons, and an indicator explaining the disposition of the claim (paid or reason for non-payment). These records were used to compute the duration of the first UI spell and to examine the importance of some of the eligibility screens.

C. WAGE RECORDS

Quarterly wage records were collected for each individual in the sample for quarters 1986.3 through 1988.1. Individuals with multiple employers in some quarters have multiple wage records for those quarters. Each wage record includes the year and quarter, an employer identifier, the amount of wages, and the number of weeks worked in the quarter. These records were used to construct variables for total wages and weeks worked per quarter following an individual's date of claim and a variable indicating if an individual returned to his/her pre-UI employer.

D. EMPLOYMENT SERVICE DATA

Data on ES registration, characteristics of registrants, and ES service receipt were collected from the Employment Service data base. These data were used to construct variables describing the services received from the ES.

E. JTPA DATA

Data on JTPA certification and JTPA service receipt were collected from the JTPA data base. These data were used to construct a set of variables describing the services received through JTPA.

F. FOLLOW-UP INTERVIEW DATA

A follow-up interview was administered to a subset of claimants (see Appendix B for a description). This interview included data on the pre-UI job, job-search activities, experience with the demonstration treatments, post-UI employment, spouse employment, other post-layoff income, demographic characteristics not available from administrative records, and work history. These variables were used in the analysis directly and to construct summary variables describing, primarily, post-UI employment and earnings.

APPENDIX B

SURVEY RESULTS AND NONRESPONSE ANALYSIS

The survey data used in this report were collected during the period from January 6 through August 3, 1988. The survey was a mixed-mode data collection, in which sample members who could not be contacted by telephone were contacted, if possible, in person. This appendix presents the results of this survey and examines potential nonresponse bias that could arise with use of these data.

A. SURVEY RESULTS

This section summarizes the results of the survey and the outcomes of the telephone and field efforts, and discusses the major reasons for nonresponse.

1. Overall Results

The results of the survey are presented in Tables B.1 and B.2. Of the 8,195 sample members on whom interviews were attempted, 5,828 responded to an interview, yielding an overall response rate of 71 percent. Not surprisingly, the propensity of sample members to respond to an interview paralleled their degree of participation in and receipt of services from the program: only 62 percent of those who had been assigned to a treatment group but had not participated in any program activities responded; this was the lowest response rate of any sample group, including noneligibles. The response rate rose to 68 percent for those treatment group members who had attended the orientation session but went no further in the program, it rose further to 74 percent for those whose final program activity was the assessment and counseling interview, and it was highest (82 percent) for those who received training or relocation assistance or a reemployment bonus from the program. The response rate for the control group sample was roughly equivalent to that for the overall treatment sample: of the 2,055 control group sample members for whom interviews were attempted, 1,469, or about 72 percent, responded. As expected, the noneligible sample group's response rate trailed behind that obtained for the treatment and control group samples; only 468 of the 743 noneligible sample members for whom interviews were attempted responded, for a response rate of 63 percent.

2. Telephone Interviewing Results

As mentioned above, an initial attempt was made to interview sample members by telephone, and of the 5,828 interviews that were completed, 91 percent (Table B.2) were administered from the telephone center. The overall response rate achieved by the telephone center was 65 percent (5,285 completed interviews out of the total release of 8,195), indicating that the overall response rate would have been reduced by about 6 percentage points (at the maximum), if sample members who could not be contacted by telephone had not been pursued in the field.

TABLE B.1

RESULTS OF SURVEY BY TREATMENT CONTROL STATUS

	Treatments													
	Did not Attend Orientation		Attended Orientation But Not Assessment		Attended Assessment		Added Services		Existing Services		Noneligibles		Total	
	No.	Percent	No.	Percent	No.	Percent	No.	Percent	No.	Percent	No.	Percent	No.	Percent
Interview Completed	520	62.1	483	67.8	2,434	73.9	454	82.3	1,469	71.5	468	63.0	5,828	71.1
Interview Refused[a]	54	6.4	40	5.6	163	5.0	12	2.2	106	5.2	39	5.3	414	5.1
Sample Member Not Contacted/Not Located[b]	212	25.3	147	20.7	586	17.8	80	14.5	395	19.2	198	26.6	1,618	19.7
Sample Member Claimed Did Not File for UI	10	0.1	5	0.7	6	0.2	0	0.0	12	0.6	7	0.9	40	0.5
Other Outcome Statuses[c]	42	5.0	37	5.2	105	3.2	6	1.1	73	3.6	31	4.2	295	3.6
Total Sample Released	838	100.0	712	100.0	3,295	100.0	552	100.0	2,055	100.0	743	100.0	8,195	100.0

[a] Includes partially completed interviews that were terminated because of a refusal to continue, proxy refusals at the sample members' request, and cases in which access to the sample member was denied.

[b] Includes sample members who could not be located and who could be located but not contacted, and sample assignments that were discontinued because the data collection period came to an end.

[c] Includes deceased and institutionalized sample members, sample members that were unavailable during the data collection period, and sample members who could not respond to an interview due to language barriers or physical or mental handicaps.

TABLE B.2

FINAL STATUS BY INTERVIEWING MODE

	Central Telephone Facility	Field Interviewer Telephone	In-Person	Final Statuses	
				No.	Percent
Interview Completed	5,285	176	367	5,828	71.1
Interview Refused	360	4	50	414	5.1
Sample Member Not Contacted/ Not Located	1,050	10	558	1,618	19.7
Sample Member Claimed Did Not File for UI	39	0	1	40	0.5
Other Outcome Statuses	258	2	35	295	3.6
Total Sample Resolution	6,992	192	1,011	8,195	100.0

3. Field Interviewing Results

Sample members were generally assigned for in-person interviewing when they could not be contacted by telephone.[1] Altogether, 15 percent of the total sample that was released was resolved in the field. Of the 1,203 field assignments, 543, or 45 percent, resulted in completed interviews, representing 9 percent of the 5,828 completions.

4. Nonresponse

As depicted in Tables B.1 and B.2, the most prevalent reason for interview nonresponse was an inability to locate or to contact sample members during the data collection period; altogether, 1,618 sample points, or 20 percent of the total sample release fell into this category. Of the 1,618, the locations of 470 were known with a reasonable degree of certainty, but they could not be contacted because: (1) they either did not live within a reasonable proximity of New Jersey or they lived in locations remote from field interviewers and had no telephone or an unpublished telephone number; (2) they were not at home during repeated visits by field interviewers; (3) they lived in limited-access security buildings; (4) their last known address was a post office box and they did not respond to letters requesting contact; or (5) they did not respond to a message or did not appear for an appointment. Of the remaining 1,148 sample points in the uncontactable/unlocatable category, no trace of 575 was discovered during the data collection period, and 361 of the 1,618 sample points, had moved subsequent to their UI claims, and, as a result, could not be located. Finally, 212 of the 1,618 sample points were located, but their assignments were discontinued because the data collection period came to an end.

The noneligible sample group proved to be the most mobile and difficult to locate and contact; 27 percent of this group were not found or not contacted during the data collection period. Somewhat surprisingly, control group sample members were less difficult to locate and contact than were treatment group members who had little or no involvement with the program. As expected, sample members who received training or relocation assistance or the reemployment bonus were the least difficult to locate and contact: only 15 percent could not be approached for an interview for these reasons. Finally, the uncontactable/unlocatable rate for treatment group members whose last program activity was the assessment and counseling interview roughly corresponded with that for the control group: 18 percent of this group could not be contacted or located during the data collection period.

---

[1] Exceptions to this rule were made when the telephone search led to assignment of a final status, such as deceased, in which an interview could not be completed. In addition field follow-ups were not made when the respondent was believed to live in a remote location where interviewing would have been prohibitively expensive.

The second most common reason for interview nonresponse was refusals; 414 sample points, or 5 percent of the total sample release, fell into this category. Refusals came in three ways: (1) termination of an interview in the early stages and refusal to continue upon being recontacted; (2) refusal to start an interview, repeatedly broken appointments or otherwise demonstrated evasive behavior, and (3) the sample member's household refused at the sample member's request or otherwise denied access to the sample member. Not surprisingly, the highest refusal rates were incurred among treatment group members who either had not participated in the program or had dropped out of the program in the early stages. In refusing an interview, many sample members in these groups stated that they felt the program had been an intrusion upon their private lives, that the program had imposed or had threatened to impose an unsought, unrequested, and unnecessary burden upon them, and that being asked to respond to an interview was an extension of a disagreeable process. The noneligible and control group refusal rates were similar to each other: 5 percent of each group. This refusal rate was roughly replicated by treatment group sample members who had received no further program services after the assessment and counseling interview. Many of the individuals in this sample group who refused an interview also cited the intrusiveness and burden of the program, and more than a few were distressed because they had not been selected for or had not received a desired service. Finally, as expected, treatment group sample members who had received training or relocation assistance or the reemployment bonus proved to be the most cooperative of all sample members; only 12 out of the release of 552, or 2 percent, refused to participate in the survey.

The remaining 335 sample points, or slightly less than 4 percent of the total sample release, were resolved in a variety of ways. Forty claimed that they had neither filed a claim for nor collected UI during the relevant period; 39 were deceased; 21 were incarcerated or institutionalized; 19 could not respond to an interview due to physical or mental incapacitation; 28 were unavailable for the duration of the data collection period, and 188 could not respond to an interview in either English or Spanish.

B. POTENTIAL NONRESPONSE BIAS

Nonresponse bias could potentially arise with use of the interview data because approximately 29 percent of the individuals with whom interviews were attempted did not respond to the interview. As noted above, this nonresponse occurred primarily because some potential respondents could not be located. Some potential respondents also refused to be interviewed.

To analyze the implications of this nonresponse for the analysis of survey data we used administrative data that are available for both respondents and nonrespondents. These data include both demographic and other baseline data that are used as control variables in the analysis and outcome data for UI benefits and earnings (from wage records data). Thus, unlike many such analyses, we can directly examine the implications of nonresponse for impact estimation.

Before proceeding with that analysis, one methodological issue needs to be addressed. This issue arises because the survey sample was stratified to obtain a higher proportion of interviews from individuals who participated in demonstration services than the proportion than occurred in the entire demonstration sample. Because of this design, weights were constructed for the survey respondent sample for use in the impact analysis. Each cell in the design was given a weight such that the weighted distribution within each treatment group matched that of the overall sample. In order to compare the survey respondents and nonrespondents, the nonrespondent group was also assigned weights. Again the weights were assigned by cell. These weights were assigned so that the resulting weighted distribution by treatment group was the same for nonrespondents as for respondents.

Turning to our analysis of nonresponse, we first examine differences between respondents and nonrespondents for selected baseline characteristics. These differences, which are reported in Table B.3, show that the respondents were older, and also more likely to be female and white than nonrespondents. On average, they also had higher wages in the base period, and, as a result, they had higher average weekly benefit amounts for UI. Finally, the nonrespondents were more likely to expect recall than the respondents. Many of these differences are statistically significant within the treatment and control groups. In general, these findings indicate that the respondent sample displayed the demographic characteristics of a more stable population than those of the nonrespondent sample. This is not surprising given that the main reason for survey noncompletion was a failure to locate the claimant. The higher nonresponse rate for the "other race" category arises also because of the difficulties that occurred in attempting to interview Hispanics who did not speak English.[2]

Data on UI receipt and earnings following the UI claim date (Table B.4) also show some differences between respondents and nonrespondents. For example, the proportion exhausting UI

---

[2] The other race category includes Hispanics, Asians, and Native Americans.

TABLE B.3

CHARACTERISTICS OF INTERVIEW SAMPLE
BY TREATMENT GROUP AND
SURVEY RESPONSE STATUS
(Weighted Means)

	Treatment Group							
	JSA Only		JSA Plus Training/ Relocation		JSA Plus Reemployment Bonus		Control Group	
	Responders	Nonresponders	Responders	Nonresponders	Responders	Nonresponders	Responders	Nonresponders
Demographic Variables								
Percent female	51.3	39.6	50.4	39.6	48.1	39.4	50.3	41.8
Percent black	15.0	22.7	17.4	25.0	15.4	24.2	13.2	25.5
Percent other race	16.6	26.7	16.8	30.6	17.0	30.7	18.1	31.2
Percent age < 35 years	26.8	34.1	27.5	34.1	28.9	35.4	28.3	33.1
Percent age ≥ 55 years	25.0	17.0	23.6	19.1	23.6	18.6	24.5	18.4
Base Period Employment								
Mean earnings (dollars)	19,034	16,259	18,804	16,872	19,461	15,317	19,013	15,864
Percent in durable manufacturing	18.1	18.6	18.1	18.6	19.2	18.2	19.1	18.3
Percent in nondurable manufacturing	17.3	19.7	17.9	21.2	17.2	19.8	18.2	23.0
Percent expecting recall	33.0	40.1	33.1	42.7	33.0	42.9	32.9	41.0
UI Entitlement								
Mean weekly benefit rate (dollars)	182.3	174.4	181.9	178.6	186.3	172.8	185.1	175.0
Mean potential weeks duration	25.0	24.7	25.1	24.6	25.2	24.6	25.0	24.6
Sample Size	1,047	375	1,692	602	1,152	415	1,469	540

TABLE B.4

UI AND EARNINGS OUTCOMES
BY TREATMENT GROUP AND
SURVEY RESPONSE STATUS
(Weighted Means)

	Treatment Group							
	JSA Only		JSA Plus Training/ Relocation		JSA Plus Reemployment Bonus		Control Group	
	Responders	Nonresponders	Responders	Nonresponders	Responders	Nonresponders	Responders	Nonresponders
UI Receipt								
Mean dollars paid in benefit year	3,147	3,129	3,133	3,105	3,160	3,020	3,273	3,195
Mean weeks paid in benefit year	17.4	18.0	17.3	17.7	17.0	17.4	17.8	18.4
Mean weeks paid in first spell	15.0	15.5	15.0	15.1	14.9	14.6	15.4	16.1
Exhaustion Rate	41.4	43.9	41.3	45.0	40.8	44.6	44.3	48.8
Earnings[a] (dollars)								
Quarter 1	1,903	1,268	1,775	1,426	1,931	1,342	1,831	1,213
Quarter 2	2,355	1,851	2,330	1,790	2,452	1,846	2,312	1,784
Quarter 3	2,717	2,187	2,709	2,238	2,740	2,166	2,664	2,083
Quarter 4	2,559	2,039	2,749	2,204	2,690	2,165	2,697	2,137
Sample Size	1,047	375	1,692	602	1,152	415	1,469	540

---

[a] Quarters are calendar quarters following the date of claim.

benefits and the number of weeks paid was slightly lower for the respondents than nonrespondents, while the total amount of benefits received was slightly higher due to the higher average weekly benefit. Similarly earnings tended to be higher for respondents than nonrespondents which could be expected given the differences in baseline characteristics. However, unlike the demographic differences, the differences in these outcome variables are not statistically significant.

In conclusion, the survey nonrespondents display some significantly different demographic characteristics than respondents, but these differences do not appear to affect systematically any particular treatment group or the controls. Also, the important UI and earnings analysis variables showed no significant differences between the respondent and nonrespondent groups. Ultimately, then, analysis of treatment impacts undertaken using the survey data should be free of problems due to nonresponse.

Both administrative (wage records) and interview data on employment and earnings were collected and used in the analysis of employment and earnings impacts that was presented in Chapter VI of the impact and benefit-cost report. As reported in that chapter, the estimates obtained from the two data sources differed with the interview-based impact estimates exceeding those obtained from the wage records data. Although both sets of impact estimates were presented and both were used in the benefit-cost analysis, we argued that the interview-based estimates were likely to be more reliable for the NJUIRDP evaluation than the wage records estimates. This argument was based primarily on the fact that, because the wage records data applied to calender quarters, it was not possible to use the wage records data to focus on impacts that occurred immediately following the date of claim. This was an important problem for the analysis since the analysis of UI impacts suggested that most of the impact of the demonstration occurred soon after the date of claim.

In this appendix we provide a comparison of the wage records and interview data on earnings to provide some insight into whether our decision to highlight the interview-based estimates was sensible. We make this comparison since measurement error is generally expected to be more of a problem for interview data than wage records data.[1] Non-response is also generally expected to be more of a problem for interview data than wage records data although our analysis of interview non-response in Appendix B suggests that non-response was not a major problem for analysis using this survey. Non-response could also be a potential problem for analysis using the wage records data since the earnings of any claimants employed outside the state of New Jersey or employed outside the UI-covered sector (e.g.,,he self-employed) are not captured by NJ wage records. If such individuals are included in any earnings impact analysis with zero earnings, impact estimates will be biased downward.

To examine the importance of the potential shortcomings of both data sets we examined earnings data for the fourth quarter of 1987. We chose the fourth quarter of 1987 for analysis because it was the latest quarter for which we had complete wage records data for every claimant in the demonstration.[2] The latest available quarter was used since that quarter contained the highest average earnings and thus provided the best comparison of the two data sources.

---

[1] Interview data are subject to measurement error because of faulty recall by respondents concerning employment dates or wage rates of jobs. Wage records data can also be inaccurate because of misreporting of wages or social security numbers. Conversations with NJ state staff suggest that such misreporting may affect about 5 percent of the wage records. Wage records data can also present a problem for analysis since earnings are reported when they are received as opposed to when they are earned. For example, some individuals in the sample appear to have received, after their date of claim, sizeable lump sum amounts of severance pay from their pre-UI employers. These payments could be misinterpreted as representing earnings from a post-UI job.

Table C.1 reports mean earnings from the two data sets for this quarter. These data show that for the full sample of individuals included in both data sets, measured mean earnings were $222 greater as reported by the interview data than as reported by the wage records. Using individual differences to carry out hypothesis tests, we found that reported earnings in the interview were significantly greater than reported earnings in the wage records.[3] Roughly twenty percent of the claimants reported or were assigned positive earnings in one source and zero earnings in the other, with over half of this twenty percent reporting zero earnings in the wage records.

A first step in analyzing the differences arising from the two data sets was to attempt to identify claimants for whom the wage records data provided an incomplete measure of earnings--those who found reemployment outside New Jersey and those who became self-employed. We attempted to address the issue of out-of-state employment by removing respondents who lived outside New Jersey from the sample (this was done by using the respondent's phone number). This is a rough approximation for out-of-state employment since some of the claimants who reside outside New Jersey may have found reemployment within the state while others living in the state may have found employment outside the state. Nevertheless when we removed respondents who did not list a valid New Jersey area code (they made up 9.4 percent of the sample), the mean discrepancy between measured fourth-quarter earnings in the two data sources was reduced to $103. In addition, the standard deviation of the individual differences was reduced somewhat from $3,074 to $2,958, implying a narrowing of the distribution of individual differences--fewer of the differences are substantially greater or less than zero. The removal of out-of-state residents therefore brought about noticeably greater agreement between the two sources of measured earnings. We attribute this finding to the exclusion in the wage records of workers receiving payments from employers from outside New Jersey.

When claimants who became self-employed (as reported in the interview) were also removed from the sample, the difference between mean fourth-quarter earnings in the two data sets was reduced

---

[2] We had wage records for the first quarter of 1988, but an examination of mean earnings suggested that these data were incomplete as of the date of data collection (not all employers had submitted records).

[3] An individual difference was equal to a claimant's earnings as measured in the interview minus the claimant's earnings as reported in the wage records data.

TABLE C.1

SAMPLE MEANS OF EARNINGS FOR THE FOURTH QUARTER OF 1987
(Standard Deviations in Parentheses)

	Number of Observations	Interview Data Earnings	Wage Records Data Earnings	Interview Earnings - Wage Record Earnings
Full Sample	4,941	$3,083	$2,861	$222***
		(3,279)	(3,239)	(3,074)
N.J. Area Codes only	4,477	3,090	2,988	103**
		(3,239)	(3,244)	(2,958)
Not Self-Employed	4,789	3,038	2,926	112***
		(3,173)	(3,251)	(2,888)
N.J. Area Codes and Not Self-employed	4,344	3,042	3,053	-10
		(3,121)	(3,253)	(2,745)

*Difference statistically significant at 90 percent confidence level for a two-tailed test.
**Difference statistically significant at 95 percent confidence level for a two-tailed test.
***Difference statistically significant at 99 percent confidence level for a two-tailed test.

best copy available

to $10, with the wage records measure exceeding the interview measure. A test of the individual differences revealed that the difference in this case is not statistically significant at conventional levels. The standard deviation of the individual differences was also reduced by excluding self-employed respondents from $2,958 to $2,745. Although only 3 percent of the claimants had become self-employed, their removal from the sample had as large an effect on the comparison as removal of the out-of-state cases. This occurred because, as we would expect, the self-employed represented cases with substantially greater earnings in the interview than in the wage records. The claimants with the three highest differences of this type were self-employed workers. Overall, exclusion of self-employed claimants clearly brought about greater consistency between the two sources of quarterly earnings.

For a substantial portion of the full sample, 1,246 observations, reported earnings were equal to zero in both the wage records and in the interview responses. Because we did not want our estimates to be dominated by claimants who were unemployed and therefore had zero earnings, we repeated our analysis of earnings using the sample of claimants who reported positive earnings in at least one data source. Table C.2 presents sample statistics for earnings in the fourth calendar quarter of 1987 for this sample of claimants. Exclusion of the self-employed claimants and out-of-state claimants from this sample had similar effects as those observed for the full sample analysis.

The size of the standard deviation of the differences between the two measures of quarterly earnings implied that, after these sample exclusions, there remained considerable disagreement between the two measures for a number of claimants. Because we have attempted to purge the data set of the known shortcomings inherent in the wage records, we conclude that a large portion of the remaining inconsistency probably resulted from measurement error in the interview data.[4] Although measurement error is an important problem for any survey-based data set, it is also an extremely difficult problem to address effectively. For the purposes of this appendix we simply note that the remaining error in measurement appeared to have a mean of zero, demonstrating that there existed no systematic bias in the interview measure of quarterly earnings.

In summary, we demonstrate in this section that two of the factors which were treated differently in the two data sources--earnings from out-of-state employment and earnings from self-employment--can

---

[4] We believe that the different treatment of severance pay (see above) in the two data sets was not responsible for the remaining inconsistencies because the fourth quarter of 1987 was at least one quarter later than the claim filing date for all claimants. It was likely therefore that most severance pay was received by claimants prior to this calendar quarter.

TABLE C.2

SAMPLE MEANS OF EARNINGS FOR THE FOURTH QUARTER OF 1987
OF 1987 - EXCLUDING CLAIMANTS WITH ZERO EARNINGS IN
BOTH DATA SETS
(Standard Deviations In Parentheses)

	Number of Observations	Interview Data Earnings	Wage Records Data Earnings	Interview Earnings - Wage Record Earnings
Full Sample	3,692	$4,030 (3,200)	$3,740 (3,230)	$290*** (3,074)
N.J. Area Codes only	3,395	3,980 (3,157)	3,848 (3,200)	132** (3,356)
Not Self-Employed	3,581	3,966 (3,076)	3,820 (3,222)	146*** (3,299)
N.J. Area Codes and Not Self-employed	3,297	3,913 (3,021)	3,926 (3,190)	-13 (3,113)

*Difference statistically significant at 90 percent confidence level for a two-tailed test.
**Difference statistically significant at 95 percent confidence level for a two-tailed test.
***Difference statistically significant at 99 percent confidence level for a two-tailed test.

account for a substantial portion of the inconsistency between reported earnings in the two data sets. When we attempted to control for these two factors, there no longer remained a significant difference between reported earnings in the two data sets for the fourth quarter of 1987. Some significant components of earnings therefore can only be accounted for by using the interview data, which incorporate a more inclusive measure of earnings, rather than depending upon state wage records to measure earnings.

APPENDIX D

SUPPLEMENTARY TABLES

This appendix reports means and standard deviations for the variables used in the regressions plus full regression results for the principal impact estimates presented in Chapters V and VI of the impact and benefit-cost report.

LIST OF TABLES

Table		Page
D.1	DEFINITIONS OF INDEPENDENT VARIABLES	377
D.2	DESCRIPTIVE STATISTICS FOR VARIABLES USED IN THE REGRESSION ANALYSES (Records Data Sample)	379
D.3	DESCRIPTIVE STATISTICS FOR VARIABLES USED IN THE REGRESSION ANALYSES (Interview Data Sample)	381
D.4	REGRESSION ESTIMATES OF UI BENEFIT RECEIPT EQUATIONS FOR RECORDS SAMPLE	383
D.5	REGRESSION ESTIMATES OF UI BENEFIT RECEIPT EQUATIONS FOR INTERVIEW SAMPLE WITH INTERACTION TERMS	385
D.6	REGRESSION ESTIMATES OF EMPLOYMENT EQUATIONS	393
D.7	REGRESSION ESTIMATES OF EARNINGS EQUATIONS	395
D.8	REGRESSION ESTIMATES OF EARNINGS EQUATION WITH INTERACTION TERMS	397
D.9	REGRESSION ESTIMATES OF EARNINGS EQUATIONS WITH INTERACTION TERMS	405

TABLE D.1

DEFINITIONS OF INDEPENDENT VARIABLES

Variable Name[a]	Definition
Treatment 1	= 1 if member of treatment group 1 = 0 otherwise
Treatment 2	= 1 if member of treatment group 2 = 0 otherwise
Treatment 3	= 1 if member of treatment group 3 = 0 otherwise
Female	= 1 if female = 0 if male
Black	= 1 if black = 0 otherwise
Other Races	= 1 if Hispanic, Asian or Native American = 0 otherwise
Age 25-34 Years	= 1 if age at date of claim was 25-34 years = 0 otherwise
Age $\geq$ 55 Years	= 1 if age at date of claim was 55 or older = 0 otherwise
Base Period Earnings	Sum of earnings in base period in thousands of dollars
Durable Manufacturing	= 1 if main base period employer in nondurable manufacturing = 0 otherwise
Nondurable Manufacturing	= 1 if main base period employer in nondurable manufacturing = 0 otherwise
Union Hiring Hall User	= 1 if reported use of union hiring hall = 0 otherwise
Expect Recall	= 1 if expected recall = 0 otherwise
Potential Duration	Weeks of potential UI receipt (entitlement/weekly benefit amount)

TABLE D.1 (continued)

Variable Name[a]	Definition
Weekly Benefit Amount	Maximum weekly UI payment for the claimant
High School Graduate	= 1 if high school graduate = 0 otherwise
College Graduate	= 1 if college graduate = 0 otherwise
Prior Experience with UI	= 1 if reported collection of UI benefits prior to date of claim = 0 otherwise
Clerical Occupation	= 1 if pre-UI occupation was clerical = 0 otherwise
Other White Collar Occupation	= 1 if pre-UI occupation was white collar other than clerical = 0 otherwise
Married	= 1 if married = 0 otherwise
Married, Spouse Employed	= 1 if married and spouse employed = 0 otherwise

[a] Additional dummy variables for cohort and office were used. Cohorts were defined for the four quarters of sample selection (July 1986-June 1987). For example, claimants who were selected during the July 1986 to September 1986 period were in cohort 1.

TABLE D.2

DESCRIPTIVE STATISTICS FOR VARIABLES USED IN THE REGRESSION ANALYSES

(Records Data Sample)

	Mean	Standard Deviation
Dependent Variables		
Dollars Paid in Benefit Year	3,132.13	1,835.04
Weeks Paid in Benefit Year	17.44	8.71
Weeks Paid in First Spell	15.03	9.27
Exhaustion Rate	0.43	0.50
Independent Variables		
Treatment 1	0.22	0.41
Treatment 2	0.34	0.48
Treatment 3	0.22	0.42
Female	0.48	0.50
Black	0.17	0.38
Other Races	0.22	0.46
Age 25-34 Years	0.30	0.46
Age ≥ 55 Years	0.22	0.41
Base Period Earnings (1000s of dollars)	18.05	11.81
Durable Manufacturing	0.18	0.39
Nondurable Manufacturing	0.20	0.40
Union Hiring Hall User	0.06	0.24
Expect Recall	0.36	0.48
Potential Duration (weeks)	24.94	2.98
Weekly Benefit Amount (dollars)	180.75	47.34
Quarter 1 Cohort	0.24	0.43
Quarter 2 Cohort	0.26	0.44
Quarter 3 Cohort	0.25	0.43
Office		
Paterson	0.09	0.29
Hackensack	0.11	0.31
Jersey City	0.10	0.30
Butler	0.07	0.25
Bloomfield	0.10	0.30
Newark	0.12	0.33
Perth Amboy	0.11	0.31
Burlington	0.09	0.29
Deptford	0.09	0.29
Sample Size	11,060	

TABLE D.3

DESCRIPTIVE STATISTICS FOR VARIABLES USED IN THE REGRESSION ANALYSES

(Interview Data Sample)

	Mean	Standard Deviation
Dependent Variables		
UI Variables		
Dollars Paid in Benefit year	3,179	1,876
Weeks Paid in Benefit Year	17.39	8.76
Weeks Paid in First Spell	15.08	9.33
Exhaustion Rate	0.42	0.49
Employment and Earnings		
Proportion of Time Employed		
Quarter 1	15.9	26.5
Quarter 2	42.6	45.5
Quarter 3	58.4	46.1
Quarter 4	64.3	45.3
Earnings		
Quarter 1	773	1,588
Quarter 2	2,085	2,922
Quarter 3	2,788	3,168
Quarter 4	3,045	3,268
Independent Variables		
Treatment 1	0.19	0.40
Treatment 2	0.32	0.47
Treatment 3	0.22	0.41
Female	0.50	0.50
Black	0.15	0.36
Other Races	0.17	0.38
Age 25-34 years	0.28	0.45
Age ≥ 55 Years	0.24	0.43
Base Period Earnings (1000s of dollars)	19.05	12.34
Durable Manufacturing	0.19	0.39
Nondurable Manufacturing	0.18	0.38
Union Hiring Hall User	0.05	0.22
Expect Recall	0.33	0.47
Potential Duration (weeks)	25.08	2.74
Weekly Benefit Amount (dollars)	183.80	46.90
High School Graduate	0.73	0.45
College Graduate	0.14	0.35

TABLE D.3 (continued)

	Mean	Standard Deviation
Prior Experience with UI	0.58	0.49
Clerical Occupation	0.18	0.38
Other White Collar Occupation	0.28	0.48
Married	0.64	0.48
Married, Spouse Employed	0.44	0.50
Cohort 1	0.24	0.43
Cohort 2	0.26	0.44
Cohort 3	0.24	0.43
Paterson	0.09	0.28
Hackensack	0.11	0.32
Jersey City	0.08	0.26
Butler	0.07	0.26
Bloomfield	0.11	0.31
Newark	0.09	0.29
Perth Amboy	0.12	0.33
Burlington	0.11	0.31
Deptford	0.10	0.31
Sample Size	5,360	

TABLE D.4

RECRESSION ESTIMATES OF UI BENEFIT RECEIPT EQUATIONS FOR RECORDS SAMPLE

(Standard Errors in Parentheses)

Explanatory Variables	Dependent Variable			
	Benefits Paid	Number of Weeks Paid in Benefit Year	Number of Weeks Paid in First Spell	Exhaustion Rate
Constant	-1,570.9** (165.2)	6.52** (0.88)	9.01*** (0.94)	0.909*** (0.050)
Treatment 1	-86.9* (45.5)	-0.47* (0.24)	-0.59** (0.26)	-0.028** (0.014)
Treatment 2	-81.1** (41.2)	-0.48** (0.22)	-0.53** (0.23)	-0.017 (0.013)
Treatment 3	-169.8*** (45.4)	-0.97*** (0.24)	-0.93** (0.26)	-0.037*** (0.014)
Female	164.2*** (33.8)	0.77*** (0.18)	0.25 (0.19)	0.039*** (0.010)
Black	260.3*** (47.0)	1.34*** (0.25)	1.28** (0.27)	0.079*** (0.014)
Other Races	77.2* (45.8)	0.34 (0.24)	-0.47* (0.26)	0.011 (0.014)
Age 25-34 Years	-1.6 (35.4)	-0.07 (0.19)	0.04 (0.20)	0.003 (0.011)
Age $\geq$ 55 Years	249.0*** (39.4)	1.23*** (0.21)	0.86*** (0.22)	0.085*** (0.012)
Base Period Earnings (1000s of dollars)	-9.5*** (1.95)	-0.07*** (0.01)	-0.04*** (0.01)	-0.003*** (0.001)
Durable Manufacturing	136.8*** (41.0)	0.66*** (0.22)	0.36* (0.23)	0.045*** (0.012)
Nondurable Manufacturing	-11.0 (41.6)	-0.21 (0.22)	-1.03*** (0.24)	-0.044*** (0.013)
Union Hiring Hall User	-124.3* (64.3)	-0.74*** (0.35)	-0.92** (0.39)	-0.027 (0.020)
Expect Recall	-502.8*** (35.1)	-2.67*** (0.19)	-3.72*** (0.20)	-0.184*** (0.011)
Potential Duration	59.5*** (5.4)	0.37*** (0.03)	0.23*** (0.03)	-0.023*** (0.002)
Weekly Benefit Amount (in hundreds)	1,900.0*** (44.0)	1.69*** (0.24)	1.54*** (0.25)	0.089*** (0.013)

TABLE D.4 (continued)

Explanatory Variables	Dependent Variable Benefits Paid	Number of Weeks Paid in Benefit Year	Number of Weeks Paid in First Spell	Exhaustion Rate
Cohort 1	-40.0	-0.13	-0.58**	0.008
	(43.5)	(0.23)	(0.25)	(0.013)
Cohort 2	129.4***	0.95***	0.70***	0.049***
	(42.6)	(0.23)	(0.24)	(0.013)
Cohort 3	-132.9***	-0.57***	-0.52**	-0.044***
	(42.8)	(0.23)	(0.24)	(0.013)
Paterson	40.4	0.51	-0.11	-0.011
	(66.6)	(0.36)	(0.38)	(0.020)
Hackensack	39.5	0.28	0.24	0.023
	(63.4)	(0.34)	(0.36)	(0.019)
Jersey City	263.4***	1.81***	0.88**	0.096***
	(65.8)	(0.35)	(0.37)	(0.020)
Butler	-119.5	-0.69*	-0.69	-0.036
	(74.1)	(0.40)	(0.42)	(0.022)
Bloomfield	107.8*	0.51	0.28	0.028
	(64.9)	(0.35)	(0.37)	(0.020)
Newark	28.8	0.26	0.33	0.036*
	(63.7)	(0.34)	(0.36)	(0.019)
Perth Amboy	81.2	0.53	0.28	0.008
	(62.8)	(0.34)	(0.36)	(0.019)
Burlington	-287.5***	-1.42***	-1.67***	-0.070***
	(66.8)	(0.36)	(0.38)	(0.020)
Deptford	-7.3	0.20	0.58	-0.006
	(67.9)	(0.36)	(0.38)	(0.021)
R^2	.264	.064	.074	.072
F-statistic	146.2	27.8	32.6	31.9
Degrees of Freedom	(27, 11,032)	(27, 11,032)	(27, 11,032)	(27, 11,032)

NOTE: The records data sample was used for these estimates.

* Statistically significant at 90 percent confidence level for a two-tailed test.
** Statistically significant at 95 percent confidence level for a two-tailed test.
*** Statistically significant at 99 percent confidence level for a two-tailed test.

TABLE D.5

REGRESSION ESTIMATES OF UI BENEFIT RECEIPT EQUATIONS FOR INTERVIEW SAMPLE WITH INTERACTION TERMS

(Standard Errors in Parentheses)

Explanatory Variables	Dependent Variable			
	Benefits Paid	Number of Weeks Paid in Benefit Year	Number of Weeks Paid in First Spell	Exhaustion Rate
Constant	-1577*** (316)	6.42*** (1.67)	7.51*** (1.76)	0.988*** (.093)
Treatment 1	299 (327)	1.27 (1.73)	1.44 (1.81)	0.070 (.096)
Treatment 2	472 (289)	2.33 (1.53)	1.02 (1.61)	0.112 (.085)
Treatment 3	-49 (320)	0.19 (1.69)	0.21 (1.78)	-0.027 (.095)
Female	139 (98)	0.70 (0.52)	0.24 (0.54)	0.037 (.029)
Black	386*** (146)	2.09*** (0.77)	2.69*** (0.81)	0.153*** (.043)
Other Races	97 (136)	0.56 (0.72)	0.23 (0.76)	0.068 (.040)
Ages 25-34 Years	-25 (102)	-0.20 (0.54)	-0.38 (0.57)	-0.023 (.030)
Age ≥ 55 Years	216** (109)	0.92 (0.58)	0.38 (0.61)	0.084*** (.032)
Base Period Earnings (1000s of dollars)	-8*** (2.8)	-0.06*** (.015)	-0.04** (.015)	-0.02** (.0008)
Durable Manufacturing	155 (116)	0.69 (0.61)	0.51 (0.64)	0.046 (.034)
Nondurable Manufacturing	200 (125)	1.00 (0.66)	-0.35 (0.70)	0.025 (.037)
Union Hiring Hall User	-148 (188)	-0.74 (0.99)	-0.84 (1.04)	-0.050 (.056)
Expect Recall	-268*** (104)	-1.66*** (0.55)	-2.81*** (0.58)	-0.130*** (.031)
Potential Duration	44*** (9)	0.28*** (.05)	0.19*** (.049)	-0.030*** (.003)
Weekly Benefit Amount (in hundreds)	19*** (1)	0.02*** (.003)	0.02*** (.004)	0.001*** (.0002)

TABLE D.5 (continued)

Explanatory Variables	Dependent Variable			
	Benefits Paid	Number of Weeks Paid in Benefit Year	Number of Weeks Paid in First Spell	Exhaustion Rate
High-School Graduate	52	0.29	0.21	0.007
	(110)	(0.58)	(0.61)	(.032)
College Graduate	-170	-0.97	-1.17	-0.026
	(141)	(0.75)	(0.78)	(.042)
Prior Experience with UI	-35	-0.09	-0.52	-0.038
	(88)	(0.47)	(0.49)	(.026)
Clerical Occupation	295**	1.64**	1.96***	0.095**
	(134)	(0.71)	(0.74)	(.040)
Other White-Collar Occupation	130	0.83	0.99	0.058
	(123)	(0.65)	(0.68)	(.036)
Married	-25	-0.15	0.00	0.001
	(125)	(0.66)	(0.69)	(.037)
Married, Spouse Employed	146	0.68	0.32	0.043
	(119)	(0.63)	(0.66)	(.035)
Cohort 1	-190	-0.78	-1.27*	-0.029
	(121)	(0.64)	(0.67)	(.036)
Cohort 2	123	1.10*	0.73	0.070**
	(117)	(0.62)	(0.65)	(.035)
Cohort 3	-219*	-0.79	-0.72	-0.015
	(119)	(0.63)	(0.66)	(.035)
Patterson	-70	0.07	-0.90	-0.041
	(194)	(1.03)	(1.08)	(.057)
Hackensack	258	1.41	1.58	0.020
	(175)	(0.93)	(0.97)	(.052)
Jersey City	266	2.17**	1.58	0.070
	(196)	(1.04)	(1.09)	(.058)
Butler	78	0.60	1.11	0.026
	(198)	(1.05)	(1.10)	(.058)
Bloomfield	155	0.94	1.25	0.063
	(179)	(0.95)	(1.00)	(.053)
Newark	212	1.58	0.51	0.085
	(192)	(1.02)	(1.07)	(.057)
Perth Amboy	284*	1.77**	2.62***	0.065
	(170)	(0.90)	(0.94)	(.050)
Burlington	-364**	-2.07**	-2.12**	-0.080
	(181)	(0.96)	(1.00)	(.053)
Deptford	205	1.38	0.73	0.044
	(181)	(0.96)	(1.01)	(.053)

TABLE D.5 (continued)

Explanatory Variables	Benefits Paid	Number of Weeks Paid in Benefit Year	Number of Weeks Paid in First Spell	Exhaustion Rate
	Dependent Variable			
Interactions for Treatment 1				
Female	-69 (147)	-0.35 (0.78)	-0.61 (0.82)	-0.015 (.043)
Black	-224 (217)	-1.23 (1.15)	-2.27* (1.21)	-0.077 (.064)
Other Race	-215 (212)	-0.97 (1.12)	-1.71 (1.18)	-0.025 (.063)
Age 25-34 Years	104 (161)	0.42 (0.85)	-0.12 (0.90)	0.034 (.048)
Age ≥ 55 Years	41 (171)	0.07 (0.90)	-0.02 (0.95)	-0.012 (.051)
Durable Manufacturing	173 (180)	0.96 (0.95)	1.28 (1.00)	0.037 (.053)
Nondurable Manufacturing	-292 (193)	-1.48 (1.02)	-0.99 (1.07)	-0.130** (.057)
Union Hiring Hall User	54 (303)	0.84 (1.60)	1.49 (1.68)	0.039 (.089)
Expect Recall	-130 (159)	-0.44 (0.84)	-0.84 (0.88)	-0.068 (.047)
High School Graduate	-194 (171)	-0.95 (0.90)	-0.32 (0.95)	0.014 (.050)
College Graduate	-113 (211)	-0.10 (1.12)	0.16 (1.17)	-0.029 (.062)
Prior Experience with UI	0 (139)	-0.20 (0.73)	-0.29 (0.77)	0.042 (.041)
Clerical Occupation	-90 (206)	-0.28 (1.09)	-0.30 (1.14)	-0.080 (.061)
Other White Collar Occupation	27 (185)	-0.10 (0.98)	-0.31 (1.03)	-0.082 (.055)
Married	-3 (193)	0.19 (1.02)	0.25 (1.07)	0.016 (.057)
Married, Spouse Employed	7 (188)	0.34 (0.99)	-0.11 (1.05)	-0.032 (.056)
Cohort 1	76 (186)	0.63 (0.98)	0.66 (1.03)	0.030 (.055)
Cohort 2	1 (181)	0.18 (0.96)	0.09 (1.01)	-0.019 (.053)
Cohort 3	143 (185)	0.89 (0.98)	0.26 (1.03)	-0.007 (.055)
Patterson	-348 (302)	-2.51 (1.60)	-1.01 (1.68)	-0.050 (.089)

TABLE D.5 (continued)

Explanatory Variables	Dependent Variable			
	Benefits Paid	Number of Weeks Paid in Benefit Year	Number of Weeks Paid in First Spell	Exhaustion Rate
Hackensack	-569**	-2.62*	-3.13**	-0.104
	(266)	(1.41)	(1.48)	(.079)
Jersey City	67	0.39	0.98	0.070
	(315)	(1.67)	(1.75)	(.093)
Butler	29	-0.60	-1.01	0.054
	(318)	(1.68)	(1.77)	(.094)
Bloomfield	-83	-0.56	-1.12	-0.045
	(278)	(1.47)	(1.54)	(.082)
Newark	-345	-2.28	-0.43	-0.060
	(298)	(1.57)	(1.66)	(.088)
Perth Amboy	-181	-1.25	-3.35	-0.037
	(266)	(1.41)	(1.48)	(.079)
Burlington	168	1.47	0.83	0.034
	(282)	(1.49)	(1.57)	(.083)
Deptford	-565**	-2.97**	-2.35	-0.111
	(286)	(1.51)	(1.59)	(.084)
Interactions for Treatment 2				
Female	66	0.28	1.02	0.011
	(130)	(0.68)	(0.72)	(.038)
Black	-300	-1.72*	-2.38**	-0.108*
	(189)	(1.00)	(1.05)	(.056)
Other Race	-166	-1.05	-1.07	-0.092*
	(189)	(1.00)	(1.05)	(.056)
Age 25-34 Years	23	0.29	0.93	0.047
	(141)	(0.74)	(0.78)	(.042)
Age ≥ 55 Years	71	0.64	1.02	0.012
	(151)	(0.80)	(0.84)	(.044)
Durable Manufacturing	93	0.47	0.09	0.012
	(159)	(0.84)	(0.88)	(.047)
Nondurable Manufacturing	61	0.38	0.82	-0.042
	(171)	(0.90)	(0.95)	(.050)
Union Hiring Hall User	-361	-1.55	-1.26	-0.029
	(268)	(1.41)	(1.49)	(.079)
Expect Recall	-264*	-1.34*	-1.94**	-0.039
	(141)	(0.74)	(0.78)	(.042)
High School Graduate	-54	-0.26	0.68	-0.008
	(151)	(0.80)	(0.84)	(.044)
College Graduate	140	0.93	1.29	-0.039
	(195)	(1.03)	(1.08)	(.057)
Prior Experience with UI	-60	-0.41	0.50	0.001
	(121)	(0.64)	(0.67)	(.036)

TABLE D.5 (continued)

Explanatory Variables	Dependent Variable Benefits Paid	Number of Weeks Paid in Benefit Year	Number of Weeks Paid in First Spell	Exhaustion Rate
Clerical Occupation	-189 (184)	-1.14 (0.97)	-1.99* (1.02)	-0.037 (.054)
Other White Collar Occuation	-8 (170)	-0.29 (0.90)	-0.78 (0.95)	0.002 (.050)
Married	-94 (170)	-0.45 (0.90)	0.60 (0.94)	0.025 (.050)
Married, Spouse Employed	-17 (163)	0.20 (0.86)	-0.06 (0.90)	-0.016 (.048)
Cohort 1	104 (166)	0.63 (0.88)	0.53 (0.92)	0.014 (.049)
Cohort 2	19 (162)	0.17 (0.86)	0.50 (0.90)	-0.005 (.048)
Cohort 3	198 (166)	0.85 (0.88)	1.24 (0.92)	0.025 (.049)
Patterson	-216 (265)	-1.08 (1.40)	0.43 (1.47)	-0.041 (.078)
Hackensack	-229 (243)	-1.15 (1.28)	-1.22 (1.35)	-0.025 (.072)
Jersey City	-307 (271)	-2.05 (1.43)	-1.80 (1.51)	-0.093 (.080)
Butler	-340 (278)	-2.09 (1.47)	-2.33 (1.54)	-0.123 (.082)
Bloomfield	-541** (245)	-2.94** (1.29)	-3.74*** (1.36)	-0.166** (.072)
Newark	-515* (263)	-2.93** (1.39)	-0.81 (1.46)	-0.177** (.078)
Perth Amboy	-305 (233)	-1.74 (1.23)	-2.58** (1.29)	-0.136** (.069)
Burlington	-110 (243)	-0.62 (1.28)	-1.34 (1.35)	-0.052 (.072)
Deptford	-554** (249)	-2.88** (1.31)	-2.64* (1.38)	-0.179** (.073)
Interactions for Treatment 3				
Female	-64 (145)	-0.63 (0.76)	-0.01 (0.80)	-0.027 (.043)
Black	-230 (214)	-1.20 (1.13)	-2.22* (1.19)	-0.090 (.063)
Other Race	-40 (212)	-0.36 (1.12)	-1.14 (1.18)	-0.122* (.063)
Age 25-34 Years	351** (154)	1.85** (0.81)	1.91** (0.86)	0.116** (.046)

TABLE D.5 (continued)

Explanatory Variables	Dependent Variable			
	Benefits Paid	Number of Weeks Paid in Benefit Year	Number of Weeks Paid in First Spell	Exhaustion Rate
Age ≥ 55 Years	302*	1.42	0.94	0.025
	(167)	(0.88)	(0.93)	(.049)
Durable Manufacturing	-43	-0.11	-0.47	0.070
	(175)	(0.93)	(0.97)	(.052)
Nondurable Manufacturing	-311*	-1.53	-0.88	-0.106
	(188)	(0.99)	(1.05)	(.056)
Union Hiring Hall User	-398	-2.31	-1.58	-0.051
	(288)	(1.52)	(1.60)	(.085)
Expect Recall	-258*	-1.11	-0.95	-0.072
	(156)	(0.82)	(0.86)	(.046)
High School Graduate	-240	-1.32	-1.32	-0.018
	(169)	(0.89)	(0.94)	(.050)
College Graduate	232	1.29	0.92	-0.010
	(208)	(1.10)	(1.16)	(.061)
Prior Experience with UI	8	-0.09	0.20	0.038
	(134)	(0.71)	(0.75)	(.040)
Clerical Occupation	-363*	-1.61	-1.46	-0.066
	(207)	(1.09)	(1.15)	(.061)
Other White Collar Occupation	-319*	-1.89*	-1.75*	-0.098*
	(186)	(0.98)	(1.04)	(.055)
Married	-57	0.06	-0.87	-0.003
	(187)	(0.99)	(1.04)	(.055)
Married, Spouse Employed	179	0.69	1.68*	0.058
	(180)	(0.95)	(1.00)	(.053)
Cohort 1	160	0.39	0.81	-0.009
	(180)	(0.95)	(1.00)	(.053)
Cohort 2	225	0.74	1.18	-0.011
	(177)	(0.93)	(0.98)	(.052)
Cohort 3	340*	1.11	1.66	0.010
	(182)	(0.96)	(1.01)	(.054)
Patterson	319	1.42	1.46	0.140
	(303)	(1.60)	(1.69)	(.090)
Hackensack	-68	-0.92	-0.22	0.107
	(263)	(1.39)	(1.46)	(.078)
Jersey City	-17	-1.03	-0.93	0.057
	(298)	(1.57)	(1.66)	(.088)
Butler	-302	-2.07	-1.91	-0.112
	(296)	(1.56)	(1.65)	(.087)
Bloomfield	48	-0.34	0.48	0.002
	(268)	(1.42)	(1.49)	(.079)
Newark	146	0.23	2.22	0.087
	(294)	(1.55)	(1.63)	(.087)

TABLE D.5 (continued)

Explanatory Variables	Dependent Variable			
	Benefits Paid	Number of Weeks Paid in Benefit Year	Number of Weeks Paid in First Spell	Exhaustion Rate
Perth Amboy	-262 (256)	-1.88 (1.35)	-3.00** (1.42)	-0.044 (.075)
Burlington	197 (275)	1.25 (1.45)	1.73 (1.53)	0.033 (.081)
Deptford	85 (273)	0.53 (1.44)	1.36 (1.52)	0.011 (.081)
R^2	.27	.08	.10	.09
F-Statistic	16.7	3.8	5.1	4.6
Degrees of Freedom	(118, 5,239)	(118, 5,239)	(118, 5,238)	(118, 5,239)

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE D.6

REGRESSION ESTIMATES OF EMPLOYMENT EQUATIONS

(Standard Errors in Parentheses)

Explanatory Variables	Dependent Variable			
	Proportion of Time Employed in			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Constant	11.0**	37.9***	50.8***	39.4***
	(4.3)	(7.3)	(7.4)	(8.0)
Treatment 1	2.3**	4.9***	4.2**	2.8
	(1.1)	(1.8)	(1.8)	(2.0)
Treatment 2	1.9**	2.8*	2.2	1.7
	(0.9)	(1.6)	(1.6)	(1.7)
Treatment 3	2.8***	5.0***	2.3	0.6
	(1.0)	(1.7)	(1.8)	(1.9)
Female	-3.0***	-5.7***	-8.3***	-6.5***
	(0.8)	(1.4)	(1.5)	(1.5)
Black	-2.9**	-7.5***	-3.6*	-1.8
	(1.2)	(2.0)	(2.0)	(2.1)
Other Races	-0.4	-1.3	0.8	3.0
	(1.2)	(2.0)	(2.0)	(2.1)
Ages 25-34 Years	-0.8	-2.3	-2.6*	-2.7*
	(0.9)	(1.5)	(1.5)	(1.6)
Age ≥ 55 Years	-4.0***	-11.5***	-17.2***	-22.1***
	(0.9)	(1.6)	(1.6)	(1.7)
Base Period Earnings (1000s of dollars)	0.144***	0.228***	0.107	0.115
	(.045)	(.076)	(.077)	(.083)
Durable Manufacturing	-1.7	-5.9***	-6.5***	-6.2***
	(1.0)	(1.6)	(1.7)	(1.7)
Non-Durable Manufacturing	2.2	3.1*	1.4	-0.5
	(1.0)	(1.8)	(1.8)	(1.9)
Union Hiring Hall User	8.8***	6.9**	2.0	-0.3
	(1.7)	(2.8)	(2.8)	(2.9)
Expect Recall	6.0***	17.2***	14.2***	8.1***
	(0.9)	(1.5)	(1.5)	(1.6)
Potential Duration	0.36**	0.44*	0.43*	0.82***
	(0.14)	(0.24)	(0.24)	(0.26)
Weekly Benefit Amount (in hundreds)	-.044***	-.067***	-.019	.017
	(.011)	(.018)	(.018)	(.019)
High-School Graduate	1.1	0.2	0.8	1.9
	(0.9)	(1.6)	(1.6)	(1.6)
College Graduate	-0.06	1.6	2.5	1.1
	(1.2)	(2.1)	(2.1)	(2.2)

TABLE D.6 (continued)

Explanatory Variables	Dependent Variable Proportion of Time Employed in Quarter 1	 Quarter 2	 Quarter 3	 Quarter 4
Prior Experience with UI	0.9 (0.8)	2.5* (1.3)	3.7*** (1.3)	4.2*** (1.4)
Other White-Collar Occupation	0.6 (1.1)	4.4** (1.8)	5.0*** (1.8)	7.0*** (1.9)
Clerical Occupation	-1.1 (2.8)	2.6 (4.7)	-3.6 (4.7)	-9.08* (4.9)
Married	-1.8* (1.1)	-2.9* (1.8)	-2.8 (1.8)	-4.4** (1.9)
Married, Spouse Employed	0.2 (1.0)	-1.3 (1.7)	-3.0* (1.7)	-2.0 (1.8)
Cohort 1	-0.7 (1.0)	-1.4 (1.8)	-1.7 (1.8)	1.3 (2.1)
Cohort 2	-4.3*** (1.0)	-6.5*** (1.7)	-5.7*** (1.7)	-1.5 (2.0)
Cohort 3	-1.2 (1.0)	3.7** (1.7)	2.6 (1.8)	3.6* (2.1)
Paterson	2.5 (1.7)	4.9 (2.8)	6.0** (2.9)	5.9** (2.9)
Hackensack	-0.8 (1.5)	-1.2 (2.5)	-1.7 (2.5)	0.2 (2.7)
Jersey City	-0.4 (1.7)	-2.2 (2.9)	-0.5 (2.9)	0.0 (3.0)
Butler	1.5 (1.7)	1.2 (2.9)	0.7 (2.9)	2.7 (3.0)
Bloomfield	3.3** (1.5)	1.9 (2.6)	2.5 (2.6)	3.9 (2.7)
Newark	2.7* (1.6)	-0.1 (2.8)	-1.1 (2.8)	0.1 (2.9)
Perth Amboy	0.1 (1.4)	-0.4 (2.4)	0.4 (2.5)	0.3 (2.6)
Burlington	4.3*** (1.5)	8.0*** (2.6)	7.4*** (2.6)	8.8*** (2.7)
Deptford	1.7 (1.5)	4.4* (2.6)	8.3*** (2.6)	7.3*** (2.8)
R^2	.04	.07	.07	.08
F-statistic	6.9	12.1	12.5	12.1
Degrees of Freedom	(34, 5,262)	(34, 5,260)	(34, 5,254)	(34, 4,596)

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE D.7

REGRESSION ESTIMATES OF EARNINGS EQUATIONS

(Standard Errors in Parentheses)

Explanatory Variables	Dependent Variable			
	Earnings in			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Constant	473* (252)	1,382*** (447)	1,961*** (471)	1,722*** (531)
Treatment 1	125** (62)	263** (111)	171 (116)	49 (129)
Treatment 2	82 (55)	103 (98)	83 (103)	77 (112)
Treatment 3	160*** (60)	278*** (107)	131 (113)	22 (124)
Female	-292*** (50)	-718*** (88)	-947*** (93)	-887*** (101)
Black	-217*** (68)	-565*** (121)	-424*** (127)	-317** (136)
Other Races	-50 (69)	-152 (122)	-103 (129)	20 (138)
Ages 25-34 Years	-25 (52)	-12 (92)	-104 (96)	-94 (104)
Age ≥ 55 Years	-300*** (55)	-807*** (98)	-1,180*** (103)	-1,434*** (111)
Base Period Earnings (1000s of dollars)	26.8*** (2.6)	55.0*** (4.7)	58.5*** (4.9)	70.4*** (5.5)
Durable Manufacturing	-87 (57)	-294*** (101)	-358*** (107)	-356*** (114)
Non-Durable Manufacturing	20 (61)	28 (108)	-18 (113)	-44 (122)
Union Hiring Hall User	451*** (97)	494** (173)	226 (182)	151 (194)
Expect Recall	285*** (52)	826*** (91)	780*** (96)	541*** (103)
Potential Duration	-2.8 (8.3)	-16.6 (14.7)	-28.1* (15.4)	-23.4 (17.1)
Weekly Benefit Amount (in hundreds)	-0.92 (0.62)	0.16 (1.10)	2.75** (1.16)	3.02** (1.28)
High-School Graduate	54 (54)	70 (96)	141 (101)	224** (108)
College Graduate	123* (71)	529*** (127)	814*** (133)	790*** (145)

TABLE D.7 (continued)

Explanatory Variables	Dependent Variable Earnings In Quarter 1	Quarter 2	Quarter 3	Quarter 4
Prior Experience with UI	91** (44)	129* (78)	241*** (82)	329*** (89)
Other White-Collar Occupation	34 (63)	384*** (111)	525*** (117)	707*** (127)
Clerical Occupation	-74 (161)	-322 (286)	-769** (302)	-1,189*** (319)
Married	-61 (62)	-99 (110)	47 (115)	80 (125)
Married, Spouse Employed	0 (59)	-48 (105)	-275** (111)	-325*** (120)
Cohort 1	-2 (61)	-12 (108)	11 (114)	166 (138)
Cohort 2	-172*** (59)	-239** (105)	-183* (110)	-51 (134)
Cohort 3	-48 (60)	251** (107)	247** (112)	310** (136)
Paterson	129 (98)	257 (173)	197 (183)	174 (194)
Hackensack	25 (87)	119 (154)	103 (162)	210 (176)
Jersey City	-1 (100)	-34 (177)	-53 (186)	-22 (199)
Butler	114 (101)	137 (178)	114 (188)	90 (199)
Bloomfield	119 (89)	15 (158)	39 (166)	132 (181)
Newark	138 (96)	148 (171)	56 (180)	59 (191)
Perth Amboy	54 (85)	129 (150)	160 (158)	-17 (172)
Burlington	236*** (89)	553*** (158)	490*** (167)	527*** (181)
Deptford	92 (91)	103 (161)	118 (169)	79 (185)
R^2	.08	.15	.20	.23
F-statistic	13.6	26.8	37.8	40.6
Degrees of Freedom	(34, 5,262)	(34, 5,260)	(34, 5,254)	(34, 4,596)

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE D.8

REGRESSION ESTIMATES OF EMPLOYMENT EQUATIONS WITH INTERACTION TERMS

(Standard Errors in Parentheses)

Explanatory Variables	Dependent Variable			
	Proportion of Time Employed in			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Constant	14.9*** (5.1)	47.2*** (8.7)	60.7*** (8.8)	53.6*** (10.0)
Treatment 1	-0.6 (5.3)	4.3 (9.0)	-0.6 (9.1)	-6.6 (10.3)
Treatment 2	-2.6 (4.7)	-5.8 (8.0)	-13.3* (8.0)	-22.5** (9.2)
Treatment 3	-4.6 (5.2)	2.5 (8.8)	-9.2 (8.9)	-17.9* (10.1)
Female	-2.7* (1.6)	-7.2*** (2.7)	-10.5*** (2.7)	-7.9*** (3.0)
Black	-5.6** (2.4)	-12.5*** (4.0)	-3.4 (4.0)	-2.9 (4.4)
Other Races	-3.3 (2.2)	-3.6 (3.7)	-1.7 (3.8)	-1.9 (4.1)
Ages 25-34 Years	1.0 (1.7)	-0.1 (2.8)	-1.0 (2.8)	-3.0 (3.1)
Age $\geq$ 55 Years	-2.3 (1.8)	-11.5*** (3.0)	-19.4*** (3.0)	-24.0*** (3.4)
Base Period Earnings (1000s of dollars)	.130*** (.045)	.203*** (.076)	.100 (.077)	.113 (0.83)
Durable Manufacturing	-0.9 (1.9)	-3.4 (3.2)	-5.8* (3.2)	-6.4* (3.5)
Nondurable Manufacturing	1.0 (2.0)	-1.3 (3.4)	-1.3 (3.5)	-4.1 (3.8)
Union Hiring Hall User	6.4* (3.0)	6.6 (5.1)	4.1 (5.2)	7.1 (5.5)
Expect Recall	3.5** (1.7)	12.7*** (2.8)	11.0*** (2.9)	5.6* (3.1)
Potential Duration	.353** (.141)	.461* (.239)	.415* (.242)	.806*** (.262)
Weekly Benefit Amount (in hundreds)	-.039*** (.011)	-.056*** (.018)	-.011 (.018)	.024 (.020)

TABLE D.8 (continued)

Explanatory Variables	Dependent Variable Proportion of Time Employed in Quarter 1	Quarter 2	Quarter 3	Quarter 4
High-School Graduate	0.9 (1.8)	1.0 (3.0)	-0.9 (3.1)	-0.1 (3.3)
College Graduate	5.2** (2.3)	7.2* (3.9)	5.4 (3.9)	1.2 (4.3)
Prior Experience with UI	0.1 (1.4)	2.8 (2.4)	2.4 (2.5)	0.8 (2.7)
Clerical Occupation	-5.6*** (2.2)	-5.0 (3.7)	-1.5 (3.7)	-2.7 (4.1)
Other White-Collar Occupation	-3.6* (2.0)	-1.0 (3.4)	0.8 (3.4)	2.9 (3.8)
Married	-1.6 (2.0)	-2.1 (3.4)	-2.0 (3.5)	-4.6 (3.8)
Married, Spouse Employed	1.0 (1.9)	-0.7 (3.3)	-2.8 (3.3)	-2.8 (3.6)
Cohort 1	0 (2.0)	-2.2 (3.3)	-1.1 (3.4)	3.0 (4.8)
Cohort 2	-5.8*** (1.9)	-10.4** (3.2)	-10.0*** (3.2)	-5.3 (4.7)
Cohort 3	-0.8 (1.9)	3.0 (3.3)	0.8 (3.3)	3.6 (4.7)
Paterson	1.0 (3.2)	5.0 (5.3)	1.6 (5.4)	-0.2 (5.7)
Hackensack	-4.9* (2.8)	-4.9 (4.8)	-6.6 (4.9)	-7.9 (5.3)
Jersey City	-0.1 (3.2)	2.9 (5.4)	1.2 (5.5)	1.5 (5.9)
Butler	0.7 (3.2)	-5.8 (5.4)	-11.2** (5.5)	-8.8 (5.9)
Bloomfield	0.3 (2.9)	-1.1 (4.9)	-1.5 (5.0)	-1.0 (5.5)
Newark	0.8 (3.1)	-4.2 (5.3)	-10.0* (5.4)	-8.9 (5.7)
Perth Amboy	-1.8 (2.8)	-4.4 (4.7)	-6.4 (4.7)	-8.1 (5.2)
Burlington	4.0 (2.9)	8.7* (5.0)	2.8 (5.0)	-0.8 (5.6)
Deptford	-1.8 (2.9)	0.3 (5.0)	5.7 (5.0)	2.7 (5.5)

TABLE D.8 (continued)

Explanatory Variables	Dependent Variable Proportion of Time Employed in Quarter 1	 Quarter 2	 Quarter 3	 Quarter 4
Interactions for Treatment I				
Female	2.5 (2.4)	2.5 (4.0)	2.4 (4.1)	-0.1 (4.4)
Black	3.2 (3.5)	2.4 (6.0)	-1.8 (6.1)	-1.9 (6.4)
Other Race	6.4* (3.4)	0.6 (5.8)	0 (5.9)	2.3 (6.2)
Age 25-34 Years	-3.0 (2.6)	-2.7 (4.4)	-3.0 (4.5)	0.1 (4.8)
Age ≥ 55 Years	-2.8 (2.8)	-2.8 (4.7)	3.1 (4.8)	6.2 (5.2)
Durable Manufacturing	-2.8 (2.9)	-3.1 (4.9)	4.5 (5.0)	2.4 (5.3)
Nondurable Manufacturing	0.7 (2.0)	5.9 (5.3)	7.3 (5.4)	5.2 (5.7)
Union Hiring Hall User	5.1 (4.9)	0.1 (8.3)	-1.3 (8.4)	-4.4 (8.5)
Expect Recall	4.8* (2.6)	8.3* (4.4)	10.3** (4.4)	10.1** (4.7)
High School Graduate	-1.6 (2.8)	-8.4* (4.7)	-6.5 (4.8)	-3.4 (5.1)
College Graduate	-5.8* (3.4)	-4.6 (5.8)	-2.9 (5.9)	-2.9 (6.4)
Prior Experience with UI	2.6 (2.2)	0.7 (3.8)	4.2 (3.9)	5.2 (4.1)
Clerical Occupation	4.4 (3.3)	4.8 (5.6)	6.0 (5.7)	7.9 (6.2)
Other White Collar Occupation	4.7 (3.0)	6.3 (5.1)	7.1 (5.1)	9.1 (5.6)
Married	1.1 (3.1)	-3.2 (5.3)	-2.8 (5.4)	-3.6 (5.8)
Married, Spouse Employed	-0.1 (3.0)	7.1 (5.1)	6.9 (5.2)	10.1* (5.6)
Cohort 1	-1.3 (3.0)	1.7 (5.1)	-0.7 (5.2)	-5.1 (6.6)
Cohort 2	4.7 (2.9)	3.1 (5.0)	3.1 (5.0)	2.0 (6.5)
Cohort 3	3.4 (3.0)	6.0 (5.1)	3.8 (5.1)	-3.0 (6.5)
Patterson	-3.8 (4.9)	-3.0 (8.3)	-6.9 (8.4)	-1.8 (8.8)

TABLE D.8 (continued)

Explanatory Variables	Dependent Variable Proportion of Time Employed in Quarter 1	 Quarter 2	 Quarter 3	 Quarter 4
Hackensack	7.7*	9.3	0.1	2.1
	(4.3)	(7.3)	(7.4)	(7.9)
Jersey City	-8.3	-6.2	-7.9	-9.8
	(5.1)	(8.6)	(8.7)	(9.2)
Butler	-7.2	-1.9	1.3	0.4
	(5.1)	(8.7)	(8.8)	(9.2)
Bloomfield	1.6	0.1	-2.9	-2.6
	(4.5)	(7.6)	(7.7)	(8.3)
Newark	-3.2	0.5	-2.7	-5.1
	(4.8)	(8.2)	(8.3)	(8.5)
Perth Amboy	-0.5	3.1	-1.5	2.2
	(4.3)	(7.3)	(7.4)	(7.9)
Burlington	-5.1	-7.9	-1.2	1.7
	(4.6)	(7.7)	(7.8)	(8.5)
Deptford	-2.0	-2.6	-2.0	-3.6
	(4.6)	(7.8)	(7.9)	(8.6)
Interactions for Treatment 2				
Female	1.6	5.1	4.3	1.1
	(2.1)	(3.6)	(3.6)	(3.8)
Black	3.2	8.6*	-0.8	3.6
	(3.1)	(5.2)	(5.2)	(5.6)
Other Race	4.1	6.0	6.0	8.6
	(3.1)	(5.2)	(5.3)	(5.5)
Age 25-34 Years	-0.2	0.2	1.6	2.3
	(2.3)	(3.9)	(3.9)	(4.1)
Age ≥ 55 Years	-0.4	5.4	6.0	0.8
	(2.4)	(4.1)	(4.2)	(4.5)
Durable Manufacturing	-1.2	-2.9	-2.4	0.2
	(2.6)	(4.3)	(4.4)	(4.6)
Nondurable Manufacturing	-1.2	3.1	0.3	2.4
	(2.8)	(4.7)	(4.8)	(5.1)
Union Hiring Hall User	6.8	1.0	-2.1	-13.4*
	(4.3)	(7.3)	(7.4)	(7.9)
Expect Recall	2.5	4.1	-0.7	-0.2
	(2.3)	(3.9)	(3.9)	(4.1)
High School Graduate	-0.9	1.4	6.8	4.1
	(2.4)	(4.1)	(4.2)	(4.4)
College Graduate	-6.2**	-8.8*	-3.1	0.6
	(3.1)	(5.3)	(5.4)	(5.8)
Prior Experience with UI	1.6	1.3	3.1	5.0
	(2.0)	(3.3)	(3.4)	(3.6)

TABLE D.8 (continued)

Explanatory Variables	Dependent Variable Proportion of Time Employed in Quarter 1	 Quarter 2	 Quarter 3	 Quarter 4
Clerical Occupation	2.2 (3.0)	-0.4 (5.0)	-4.4 (5.1)	4.5 (5.5)
Other White Collar Occupation	3.2 (2.8)	5.0 (4.7)	2.1 (4.7)	2.4 (5.1)
Married	-0.7 (2.7)	-4.4 (4.7)	-4.8 (4.7)	-0.2 (5.0)
Married, Spouse Employed	1.0 (2.6)	1.6 (4.5)	1.1 (4.5)	0.9 (4.8)
Cohort 1	-1.8 (2.7)	-0.2 (4.6)	-1.4 (4.6)	-1.6 (6.0)
Cohort 2	-0.5 (2.6)	5.3 (4.4)	6.0 (4.5)	4.4 (5.9)
Cohort 3	-1.9 (2.7)	0.9 (4.6)	3.8 (4.6)	3.1 (6.0)
Patterson	5.8 (4.3)	5.4 (7.3)	17.3** (7.3)	18.6** (7.6)
Hackensack	2.0 (3.9)	1.9 (6.7)	9.6 (6.7)	14.9** (7.2)
Jersey City	3.1 (4.4)	-5.7 (7.5)	0.6 (7.6)	3.6 (8.0)
Butler	1.7 (4.5)	12.1 (7.6)	21.4*** (7.7)	21.2*** (8.0)
Bloomfield	6.8* (4.0)	6.2 (6.7)	6.7 (6.8)	9.5 (7.3)
Newark	4.6 (4.3)	11.8 (7.3)	22.7*** (7.4)	21.1*** (7.6)
Perth Amboy	0.3 (3.8)	2.7 (6.4)	11.4* (6.5)	15.7** (6.9)
Burlington	4.7 (3.9)	0.7 (6.7)	6.9 (6.7)	15.1** (7.3)
Deptford	9.4** (4.0)	14.2** (6.8)	9.2 (6.9)	12.6* (7.4)
Interactions for Treatment 3				
Female	-2.9 (2.3)	1.6 (4.0)	3.0 (4.0)	2.8 (4.2)
Black	3.7 (3.5)	8.9 (5.8)	3.1 (5.9)	0.7 (6.2)
Other Race	0.6 (3.4)	2.3 (5.8)	4.1 (5.9)	7.7 (6.1)
Age 25-34 Years	-6.4** (2.5)	-10.2** (4.2)	-6.9 (4.3)	-1.2 (4.5)

TABLE D.8 (continued)

Explanatory Variables	Dependent Variable			
	Proportion of Time Employed in			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Age ≥ 55 Years	-4.6*	-5.7	-0.8	2.3
	(2.7)	(4.6)	(4.6)	(4.9)
Durable Manufacturing	-0.7	-6.8	-4.9	0.1
	(2.8)	(4.8)	(4.9)	(5.1)
Nondurable Manufacturing	5.7*	8.5	4.9	8.7
	(3.0)	(5.2)	(5.2)	(5.6)
Union Hiring Hall User	0.8	1.5	-4.3	-10.6
	(4.7)	(7.9)	(8.0)	(8.4)
Expect Recall	1.5	5.5	6.0	3.4
	(2.5)	(4.3)	(4.3)	(4.5)
High School Graduate	5.6**	3.6	4.4	3.2
	(2.7)	(4.6)	(4.7)	(4.9)
College Graduate	-9.1***	-8.1	-5.3	-1.8
	(3.4)	(5.7)	(5.7)	(6.1)
Prior Experience with UI	-0.6	-3.2	-2.4	3.9
	(2.2)	(3.7)	(3.7)	(4.0)
Clerical Occupation	7.2**	0.8	-2.3	-1.2
	(3.3)	(5.7)	(5.7)	(6.2)
Other White Collar Occupation	4.7	5.2	3.1	4.0
	(3.0)	(5.1)	(5.2)	(5.5)
Married	-1.3	3.3	4.4	3.1
	(3.0)	(5.1)	(5.2)	(5.5)
Married, Spouse Employed	-3.1	-9.3*	-7.1	-4.5
	(2.9)	(4.9)	(5.0)	(5.3)
Cohort 1	0.2	3.0	1.4	-1.3
	(2.9)	(4.9)	(5.0)	(6.3)
Cohort 2	2.7	8.1*	9.5**	9.8
	(2.9)	(4.8)	(4.9)	(6.2)
Cohort 3	-2.0	-3.0	-0.1	-2.7
	(2.9)	(5.0)	(5.0)	(6.3)
Patterson	0.8	-11.2	-2.0	-0.9
	(4.9)	(8.3)	(8.4)	(8.7)
Hackensack	7.3	3.4	8.3	12.1
	(4.2)	(7.2)	(7.3)	(7.8)
Jersey City	1.4	11.4	-2.4	-3.9
	(4.8)	(8.1)	(8.3)	(8.7)
Butler	6.7	15.4*	23.0***	19.1**
	(4.8)	(8.1)	(8.3)	(8.6)
Bloomfield	2.0	4.4	11.9	10.2
	(4.3)	(7.3)	(7.4)	(8.0)
Newark	3.2	-1.4	9.7	14.9*
	(4.8)	(8.1)	(8.2)	(8.5)

TABLE D.8 (continued)

Explanatory Variables	Dependent Variable Proportion of Time Employed in Quarter 1	Quarter 2	Quarter 3	Quarter 4
Perth Amboy	8.6** (4.1)	13.0* (7.0)	19.0*** (7.1)	13.4* (7.6)
Burlington	0.1 (4.4)	3.4 (7.5)	13.0* (7.6)	18.6** (8.2)
Deptford	3.3 (4.4)	-0.4 (7.5)	0.1 (7.5)	4.9 (8.1)
R^2	.07	.09	.09	.10
F-Statistic	3.2	4.5	4.5	4.1
Degrees of Freedom	(118, 5,183)	(118, 5,183)	(118, 5,180)	(118, 4,521)

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

TABLE D.9

REGRESSION ESTIMATES OF EARNINGS EQUATIONS WITH INTERACTION TERMS

(Standard Errors in Parentheses)

Explanatory Variables	Dependent Variable			
	Earnings in:			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Constant	682**	1,511***	2,154***	2,205***
	(300)	(533)	(563)	(66)
Treatment 1	-82	244	341	-343
	(311)	(552)	(583)	(684)
Treatment 2	-116	-237	-91	-525
	(275)	(488)	(515)	(607)
Treatment 3	-355	-71	-301	-499
	(304)	(539)	(569)	(669)
Female	-231**	-654***	-1,038***	-867***
	(93)	(165)	(174)	(196)
Black	-325**	-660***	-286	-274
	(138)	(245)	(259)	(288)
Other Races	-143	-210	-145	-147
	(129)	(229)	(242)	(269)
Ages 25-34 Years	70	142	48	57
	(97)	(173)	(182)	(206)
Age $\geq$ 55 Years	-188*	-673***	-1,091***	-1,319***
	(104)	(185)	(195)	(224)
Base Period Earnings (1000s of dollars)	26***	55***	59***	72***
	(3)	(5)	(5)	(5)
Durable Manufacturing	-49	-163	-414**	-619***
	(110)	(195)	(206)	(230)
Nondurable Manufacturing	-15	-114	-122	-337
	(120)	(213)	(224)	(252)
Union Hiring Hall User	289	471	291	375
	(178)	(316)	(333)	(366)
Expect Recall	131	531***	578***	347*
	(99)	(175)	(185)	(205)
Potential Duration	-3.7	-16.9	-30.3*	-28.6*
	(8.3)	(14.7)	(15.5)	(17.3)
Weekly Benefit Amount (in hundreds)	-0.77	0.53	3.08***	3.12**
	(0.63)	(1.11)	(1.18)	(1.30)

TABLE D.9 (continued)

Explanatory Variables	Dependent Variable Earnings in: Quarter 1	Quarter 2	Quarter 3	Quarter 4
High-School Graduate	48 (105)	107 (185)	41 (196)	150 (217)
College Graduate	231 (134)	566** (238)	664*** (250)	740*** (286)
Prior Experience with UI	45 (84)	201 (149)	213 (157)	195 (178)
Clerical Occupation	-240* (127)	-243 (226)	-68 (239)	-118 (273)
Other White-Collar Occupation	-160 (117)	83 (207)	421* (219)	507** (250)
Married	-87 (119)	-136 (211)	59 (223)	340 (250)
Married, Spouse Employed	43 (114)	-16 (202)	-288 (213)	-552** (239)
Cohort 1	-2 (115)	-69 (205)	144 (217)	-44 (321)
Cohort 2	-298*** (110)	-455** (197)	-413** (208)	-614** (312)
Cohort 3	-24 (113)	146 (201)	153 (212)	-7 (315)
Patterson	42 (185)	310 (328)	127 (346)	186 (376)
Hackensack	-126 (167)	-29 (296)	146 (312)	490 (354)
Jersey City	-12 (187)	298 (332)	302 (351)	439 (391)
Butler	143 (188)	-210 (334)	-238 (352)	-105 (391)
Bloomfield	-102 (171)	-195 (303)	48 (319)	292 (367)
Newark	102 (184)	41 (326)	-164 (345)	100 (375)
Perth Amboy	95 (162)	173 (287)	244 (303)	109 (345)
Burlington	211 (172)	640* (305)	474 (323)	454 (373)
Deptford	-98 (172)	-71 (305)	206 (322)	295 (367)

TABLE D.9 (continued)

Explanatory Variables	Dependent Variable Earnings in Quarter 1	Quarter 2	Quarter 3	Quarter 4
Interactions for Treatment 1				
Female	-21 (140)	-199 (248)	-139 (262)	-319 (291)
Black	213 (207)	120 (368)	-91 (389)	77 (425)
Other Race	250 (201)	-155 (357)	-203 (377)	72 (410)
Age 25-34 Years	-298* (153)	-414 (272)	-513* (287)	-457 (319)
Age ≥ 55 Years	-191 (163)	-444 (289)	-390 (306)	-183 (342)
Durable Manufacturing	-106 (171)	7 (303)	522 (320)	663* (352)
Nondurable Manufacturing	-72 (184)	301 (326)	480 (344)	609 (376)
Union Hiring Hall User	458 (287)	256 (509)	101 (537)	85 (565)
Expect Recall	292* (152)	562** (269)	695** (284)	611* (312)
High School Graduate	-119 (163)	-477 (290)	-401 (306)	-286 (337)
College Graduate	-173 (201)	-3 (357)	56 (377)	-278 (423)
Prior Experience with UI	69 (132)	-101 (234)	93 (247)	248 (275)
Clerical Occupation	288 (195)	480 (347)	669* (366)	785* (413)
Other White Collar Occupation	305* (176)	265 (312)	77 (329)	198 (369)
Married	-27 (184)	-235 (327)	-261 (345)	-640* (382)
Married, Spouse Employed	125 (179)	552 (317)	529 (335)	820** (372)
Cohort 1	-2 (177)	-31 (314)	-289 (332)	173 (437)
Cohort 2	144 (173)	-68 (306)	12 (323)	471 (428)
Cohort 3	192 (176)	425 (312)	203 (329)	392 (433)
Patterson	-101 (288)	-148 (510)	-486 (538)	-297 (586)

TABLE D.9 (continued)

Explanatory Variables	Dependent Variable Earnings in:			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Hackensack	427*	691	1	-358
	(253)	(449)	(474)	(525)
Jersey City	-299	-474	-745	-899
	(299)	(530)	(560)	(608)
Butler	-428	-140	-392	-363
	(301)	(534)	(565)	(609)
Bloomfield	329	320	-251	-340
	(264)	(468)	(494)	(552)
Newark	-235	-69	-288	-621
	(283)	(503)	(531)	(565)
Perth Amboy	-74	-109	-694	-750
	(253)	(448)	(473)	(523)
Burlington	-124	-387	-215	-81
	(269)	(477)	(503)	(563)
Deptford	-90	-360	-600	-940
	(272)	(482)	(509)	(571)
Interactions for Treatment 2				
Female	46	122	238	-49
	(123)	(219)	(231)	(253)
Black	134	108	-261	-89
	(179)	(318)	(336)	(367)
Other Race	157	292	267	397
	(180)	(319)	(337)	(367)
Age 25-34 Years	-5	33	29	-44
	(134)	(237)	(250)	(275)
Age $\geq$ 55 Years	-91	10	56	-164
	(143)	(254)	(269)	(297)
Durable Manufacturing	-54	-149	132	496
	(151)	(268)	(283)	(307)
Nondurable Manufacturing	-107	-51	-21	298
	(163)	(290)	(306)	(335)
Union Hiring Hall User	388	100	2	-448
	(254)	(452)	(479)	(524)
Expect Recall	102	241	-98	-97
	(134)	(237)	(251)	(271)
High School Graduate	-54	-6	198	88
	(143)	(255)	(269)	(292)
College Graduate	-219	-310	83	55
	(184)	(328)	(345)	(382)
Prior Experience with UI	109	21	138	182
	(115)	(205)	(216)	(237)

TABLE D.9 (continued)

Explanatory Variables	Dependent Variable			
	Earnings in:			
	Quarter 1	Quarter 2	Quarter 3	Quarter 4
Clerical Occupation	10	-121	-260	121
	(175)	(311)	(328)	(362)
Other White Collar Occupation	78	112	-181	-48
	(162)	(287)	(303)	(335)
Married	46	-33	-172	-413
	(161)	(287)	(303)	(332)
Married, Spouse Employed	35	115	175	390
	(155)	(275)	(290)	(318)
Cohort 1	-51	71	-163	199
	(158)	(281)	(297)	(399)
Cohort 2	86	323	352	669*
	(154)	(274)	(290)	(391)
Cohort 3	-29	202	294	577
	(158)	(281)	(297)	(396)
Patterson	226	71	275	182
	(251)	(447)	(472)	(505)
Hackensack	-42	-47	-217	-552
	(231)	(411)	(433)	(475)
Jersey City	155	-278	-364	-409
	(259)	(460)	(486)	(528)
Butler	-49	561	547	335
	(263)	(468)	(494)	(531)
Bloomfield	487**	563	149	-8
	(233)	(414)	(436)	(485)
Newark	115	459	624	258
	(251)	(447)	(472)	(504)
Perth Amboy	-318	-380	-150	-7
	(222)	(394)	(416)	(459)
Burlington	185	-171	-209	-57
	(231)	(410)	(433)	(481)
Deptford	362	559	17	-100
	(236)	(420)	(442)	(489)
Interactions for Treatment 3				
Female	-229*	-166	96	19
	(137)	(243)	(257)	(280)
Black	53	224	-82	-157
	(203)	(359)	(379)	(412)
Other Race	-45	84	154	238
	(201)	(357)	(377)	(406)
Age 25-34 Years	-231	-471*	-324	-209
	(146)	(259)	(274)	(300)

TABLE D.9 (continued)

Explanatory Variables	Dependent Variable Earnings in: Quarter 1	Quarter 2	Quarter 3	Quarter 4
Age ≥ 55 Years	-212 (159)	-285 (281)	-190 (297)	-178 (328)
Durable Manufacturing	-52 (166)	-440 (295)	-327 (311)	79 (337)
Nondurable Manufacturing	317* (179)	416 (317)	207 (334)	557 (368)
Union Hiring Hall User	29 (273)	-11 (484)	-185 (511)	-397 (549)
Expect Recall	205 (148)	384 (262)	400 (277)	395 (300)
High School Graduate	281* (161)	310 (286)	485 (302)	284 (325)
College Graduate	16 (197)	281 (350)	399 (369)	37 (406)
Prior Experience with UI	-7 (128)	-259 (226)	-183 (239)	101 (263)
Clerical Occupation	366* (196)	149 (348)	-131 (368)	-146 (409)
Other White Collar Occupation	183 (177)	188 (313)	-127 (331)	-66 (364)
Married	33 (177)	296 (315)	313 (332)	-145 (363)
Married, Spouse Employed	-277 (171)	-694** (304)	-616* (321)	-167 (350)
Cohort 1	38 (171)	174 (304)	-159 (321)	126 (420)
Cohort 2	274 (167)	554* (297)	510 (314)	866** (412)
Cohort 3	-223 (172)	-89 (306)	-82 (323)	10 (419)
Patterson	31 (289)	-570 (512)	126 (540)	-204 (575)
Hackensack	247 (249)	12 (442)	57 (466)	-174 (513)
Jersey City	91 (283)	-795 (501)	-529 (529)	-752 (574)
Butler	234 (280)	819 (499)	1,057** (525)	560 (568)
Bloomfield	61 (255)	-36 (452)	92 (477)	-306 (530)
Newark	133 (280)	-263 (496)	249 (524)	-87 (565)

TABLE D.9 (continued)

Explanatory Variables	Dependent Variable Earnings in: Quarter 1	Quarter 2	Quarter 3	Quarter 4
Perth Amboy	328 (243)	526 (430)	589 (455)	155 (505)
Burlington	-98 (261)	206 (463)	606 (488)	584 (545)
Deptford	342 (259)	211 (459)	123 (484)	31 (535)
R^2	.10	.17	.21	.24
F-Statistic	5.1	8.8	11.7	12.2
Degrees of Freedom	(118, 5,179)	(118, 5,177)	(118, 5,171)	(118, 4,513)

*Statistically significant at the 90 percent confidence level for a two-tailed test.
**Statistically significant at the 95 percent confidence level for a two-tailed test.
***Statistically significant at the 99 percent confidence level for a two-tailed test.

APPENDIX E

BENEFIT-COST ANALYSIS METHODOLOGY AND ASSUMPTIONS

This appendix describes the specific methods and assumptions used to estimate the value of each program benefit and cost for the benefit-cost analysis presented in Chapter VII. The first section discusses the estimation of changes in total compensation, output, taxes, and UI benefits and other transfers. The second section summarizes the material on the estimation of program costs that is presented in more detail in the implementation and process report.

A. METHODOLOGY FOR ESTIMATION OF NET BENEFITS AND TRANSFERS

Several issues concerning the estimation of net benefits and transfers were not discussed fully in Chapter VII. These issues concern the estimation of effects of the demonstration treatments on total compensation, output, taxes, and transfers.

1. Total Compensation

The demonstration treatments were expected to lead to the earlier reemployment of the treatment group members as compared to the controls and, as a result, to lead to an increase in the total compensation earned by the claimants. Estimated impacts of the demonstration treatments on the earnings of claimants were presented in Chapter VI and these estimates were used in the benefit-cost analysis. However, these earnings impact estimates did not include additional fringe benefits earned by treatment group members (except for payments for vacation and sick leave, which we assumed were captured in the earnings figures), or additional payroll taxes paid on earnings by both the employer and employee.[1] Thus our earnings impact estimates had to be adjusted to obtain total compensation estimates.

This adjustment was done by using an estimate for the average value of fringe benefits and payroll taxes as a percentage of earnings.[2] A number of such estimates are available in the literature. One estimate from U.S. Department of Labor (1980) for low-wage workers in 1977 found that fringe benefits (largely the legally mandated benefits such as Social Security and UI) equalled, on average, nearly 18 percent of total wages and salaries.[3] The Department of Labor has stopped collecting these data, however, and the 1977 data do not reflect recent changes in mandated fringe benefits such as Social Security taxes. Another estimate from the U.S. Chamber of Commerce (1986) found that fringe benefits averaged 25 percent of total wage and salary income (excluding payments for time not worked such as vacation and sick leave).[4] Finally, data from the Survey of Current Business (Jones, 1986) indicated that average fringe benefits (again, excluding payments for time not worked) were 21 percent of wage and salary income.

---

[1] Employer and employee paid payroll taxes were included in our estimates of total compensation to claimants and in our estimates of taxes paid by claimants under the assumption that the incidence of such taxes falls on employees. See further discussion below.

[2] Data on the value of specific fringe benefits available to sample members could not be used. These data were not collected in the interview since respondents are not usually able to provide accurate information on this subject.

[3] Low-wage workers were defined as workers who earned less than $3.00 of total compensation per hour in 1977. This amount was approximately $5.50 per hour in 1986 dollars.

Given this range of estimates (18 to 25 percent), we chose the 21 percent figure because it is the more conservative of the estimates that are based on recent data, and because it is an estimate which covers a more representative group of workers than either of the other studies. The estimated value of the average increase in earnings plus fringe benefits per member of each treatment group (as compared to average earnings plus fringe benefits per member of the control group) was then used in the benefit-cost calculations.

2. Output

In order to value benefits from the standpoint of society it was necessary to estimate the value of the extra output produced by treatment group members (to the extent that they obtained jobs sooner or obtained better jobs than comparable control group members). To make this estimate, we used the competitive market assumption that the total value of the additional output was equal to the value of the additional earnings and fringe benefits earned by the treatment group members. By making this assumption we also implicitly assumed that any worker who was hired (or hired sooner) because of the demonstration was not taking a job away from another worker. This assumption seemed reasonable in the context of New Jersey's booming economy.

3. Taxes

In order to examine the distributional effects of the increased earnings created by the program, we needed to estimate the amount and burden of the taxes paid by workers and employers on the additional earnings.

One method for estimating taxes would be to consider separately the level of federal and state income taxes, property taxes, sales taxes, and payroll taxes, and to estimate how much of the increased earnings of the treatment groups would result in taxes of each type. This would be a complex process, and we did not have sufficient information on the incomes of the sample to do it accurately. Instead, we used estimates (Pechman, 1985) of the average rate of taxes paid by persons of moderate income to estimate the value of taxes. Three elements of Pechman estimates and our application of them are worth noting.

---

[4] This estimate was based on data from 1,000 firms.

First, in order to estimate the average tax rate for individuals it is necessary to make assumptions as to who really bears the burden of particular taxes (referred to as the "incidence" of a tax). Pechman's most important incidence assumption for the issues we are considering is that employees bear the burden of all payroll taxes, including those that are nominally paid by the employer. In other words, if UI and Social Security taxes were abolished, his assumption implies that wages or other fringes would increase by the same amount as these taxes, so that employers would not gain any net benefit. This assumption was incorporated into our analysis by including employer-paid payroll taxes in our estimate of fringe benefits, and also in our estimate of taxes paid by claimants.

Second, the income base used by Pechman to estimate average tax incidence is broader than the income base for the income tax. All transfer payments are included, as well as imputed rent from owner-occupied housing, and unrealized capital gains. In our application we assumed that earnings and transfer income (UI and the reemployment bonus) were the only components of income that differed among the treatment and control groups, and we used impacts on this income as the tax base for our calculations.

Finally, although we would have preferred to use estimates for 1986 or 1987, Pechman's most recent tax burden estimates are for 1985 and these were used.[5] The average tax rates under Pechman's most progressive tax incidence assumptions vary from 23.2 percent for the second lowest decile of the income distribution to 25.1 percent for the second highest decile, and under the least progressive incidence assumptions they vary from 25.6 percent for the second lowest decile to 26.1 percent for the second highest decile. Because most of the claimants in the demonstration had neither very low or very high incomes, we used an intermediate tax rate figure of 24 percent as a rough average of Pechman's estimates.

---

[5] Since the earnings impacts used in the analysis are for 1986 and 1987, it would have been appropriate to use tax incidence estimates for these years. The earlier estimates for 1985 may, in particular, be inappropriate because the changes in income tax rates incorporated in the 1986 Tax Reform Act began to be introduced in 1987.

A final tax estimation issue that we had to consider concerned the apportionment of the additional tax revenue between the Department of Labor and the rest of the government. We made this division by using the average UI tax rate for the U.S. for 1987 to estimate the portion of tax payments which go to the UI system.[6] It should be noted that this is a lower-bound estimate on the amount of taxes gained by the Labor Department as a whole, since JTPA and other Labor Department programs are funded from general revenues.

4. UI and Other Transfers

The demonstration was expected to effect the receipt of UI benefits and, potentially, the receipt of other transfers such as AFDC and Food Stamps. For the benefit-cost analysis we needed to take account of any impacts on benefit payments and administrative costs of these programs. As described in Chapter VII, we used the UI benefit impact estimates presented in Chapter V in our analysis, and we also used the nonmonetary determination impact estimates to compute net changes in administrative costs for these eligibility determinations. An additional impact on administrative cost of the UI program was also computed to take account of the effect of the reduced number of UI claims. To estimate this decrease in administrative costs, we multiplied the estimates of the program impact on weeks claimed times an estimate of the unit cost per week of processing a continued claim.[7]

We also considered impacts on other transfer programs, but we found that the use of such government transfers by both treatment and control group members was extremely low, and there were no significant differences across treatment groups in the proportion who received these transfers. The net change in these transfers and the administrative costs of these programs was thus set to zero in the benefit-cost calculations.

B. METHODOLOGY FOR ESTIMATION OF GROSS ADMINISTRATIVE COSTS

This section presents an abbreviated version of the discussion of estimation of gross costs that is presented in the implementation and process report. The methodology used to estimate the costs of services used by the control group, and the costs of JTPA services used by treatment groups 1 and 3 is presented in Chapter VII. These estimates were used to compute the net administrative costs of the treatments.

---

[6] We used the ratio of total tax payments to total wage and salary income as reported in USDOL's Handbook of Unemployment Insurance Financial Data. We used the rate for the U.S. (1.05 percent) although it was lower than the rate for New Jersey (1.42 percent). We did this to be consistent with Pechman's tax rates, which are applicable to the entire U.S..

[7] This unit cost was provided to us by the New Jersey Department of Labor.

1. Treatment Costs

The total costs of payments to claimants for relocation assistance, transportation allowances, and the reemployment bonus were obtained from state records of actual payments made. The cost of payments to training vendors for training, as recorded by JTPA staff at each site, were also available. However, some additional treatment group 2 members were recorded in the JTPA data system as having received classroom training or OJT, and for these individuals we did not have information on the amount spent on training. We therefore assumed that the average amount spent on classroom training and OJT for these persons was the same as for persons for whom we did have the records of vendor payments.

2. Operational Costs

The cost of operating the demonstration included the local office cost of providing the common set of initial services via the Employment Service, and the costs to JTPA of placing interested treatment group 2 members into training. In addition, there were costs to the Unemployment Insurance System of identifying the eligible claimants and monitoring the their compliance with the increased reporting requirements of the demonstration. Costs were incurred in the central offices of NJDOL in order to manage the demonstration, monitor the sites, provide training to staff, and issue treatment payments.

Using these demonstration operational costs to estimate the cost of providing program services on an ongoing basis required making assumptions about which components of costs would not be present in an ongoing program, and measuring the size of these costs as carefully as possible. Since a number of assumptions were required, the resulting cost estimates are subject to considerable uncertainty. For this reason, we used a conservative approach to excluding demonstration costs and then tested the sensitivity of our results to assumptions of further reductions in cost.

The analysis of operational costs used only costs incurred from October 1986 through June 1987, the period during which the demonstration was fully operational. That is, costs for the first and last three months of the demonstration were excluded, in order to approximate better costs of an ongoing program.[8] We divided costs into three categories: central office labor costs, local office labor costs, and other direct and indirect (overhead) costs.

Central Office Labor Costs. Central office staff played important roles in the planning and evaluation of the program which would probably not be present in an ongoing program. In order to exclude such costs demonstration-specific costs, we examined the functions performed by each division (or sometimes, individuals within each division) and decided whether these functions would be included in an ongoing program. We either included or excluded all demonstration costs associated with a division (or individual). In general, we included the costs of the operating divisions (UI, ES, and JTPA) and excluded the costs of other divisions.

There are two major reasons that the resulting cost measures may still not provide an accurate picture of expenditures in an ongoing program. First, some staff members included in the cost figures may have devoted more time to the demonstration than they would to an ongoing program. Second, the organizational structure of the demonstration required substantial central office coordination among ES, UI and JTPA staff. Costs might be lower in an ongoing program in which interagency linkages would be established early on, and not require extensive ongoing attention. These issues were examined in our sensitivity tests.

In summary, the base case estimate of central office costs included all expenditures incurred by a subset of central office UI, ES and JTPA staff in supervising the demonstration, as well as central office expenditures incurred in processing payments. Fringe benefit costs (at the assessed rate of 24.1 percent) were added to direct labor costs.

Local Office Labor Costs. For the UI and ES local offices, we based our estimates on expenditure records obtained from the NJDOL. Again, we attempted to exclude costs that would not be incurred in an ongoing program. In particular, the eligibility questions which were collected on a separate form by UI staff and data entered by ES staff would be included in a standard eligibility form in an ongoing program and would not require special processing. To account for this situation an estimate of the cost of processing these forms was subtracted from the total local office cost. All other UI and ES labor costs were included in the estimates. As with the central office labor costs, fringe benefit costs (at the assessed rate of 24.1 percent) were added to direct labor costs.

---

[8] JTPA administrative costs were estimated for the entire duration of the program, however, because these were the only data available.

Local JTPA labor costs, including fringe benefits, were provided to us by staff at the NJDOL. All costs charged to the demonstration by JTPA local operators were included in the base case estimate.

Other Operational Costs. In addition to labor costs, operational costs include indirect costs such as general administration and rent. These costs were estimated on the basis of the rates at which these items were assessed by the state during the period from October 1986 through June 1987. Operational costs also include direct costs other than labor. These costs were estimated using actual costs, when available, and budgeted costs otherwise. These data were adjusted when it became clear that some costs, such as travel to local offices and planning meetings, would not be as high in an ongoing program. Estimation of data processing costs was especially difficult. The demonstration had a completely separate data processing system, but we assumed that in an ongoing program data processing would be integrated into the state's existing system. As an estimate of these costs, we assumed (based on estimates by NJDOL staff) that the project would occupy half of the available time on one terminal (linked to the NJDOL mainframe) at each local office, and would thus incur one-half of the monthly per-terminal charge at each office.[9]

3. Determining Costs per Person for the Three Treatment Groups

The total costs of each component of the demonstration were allocated across the three treatment groups in order to derive per claimant estimates of the costs of providing each treatment. The procedure was as follows. All direct treatment costs for training and relocation were allocated to treatment 2, and all reemployment bonus costs were allocated to treatment 3. All ES and UI operational costs were allocated to the three treatments based on the proportion of the sample in each treatment group over the October 1986 to June 1987 period. All JTPA operational costs were allocated to treatment 2. The total costs per treatment were then divided by the number of enrollees in each treatment group over the appropriate period to obtain per claimant cost estimates.[10]

---

[9] The difference between this cost estimate and the costs of the demonstration's Microvax system was quite small--about 3 dollars per claimant--suggesting that the cost estimates were not too sensitive to this assumption.

[10] For more details on the procedures used, see the implementation and process report.

APPENDIX E

REFERENCES

Jones, David. "The Treatment of Fringe Benefits in Personal Injury and Wrongful Death Calculations." *Benefits Quarterly*, v.2, no. 4, 1986, pp. 59-62.

Pechman, Joseph. *Who Paid the Taxes, 1966-1985*. Washington, D.C.: The Brookings Institution, 1985.

U.S. Chamber of Commerce. *Employee Benefits 1985*. Washington, D.C.: U.S. Chamber of Commerce, 1986.

U.S. Department of Labor. *Employee Compensation in the Private Nonfarm Economy*. Washington, D.C.: Bureau of Labor Statistics, 1980.

UI OCCASIONAL PAPER SERIES

The Unemployment Insurance Occasional Paper Series presents research findings and analyses dealing with unemployment insurance issues. Papers are prepared by research contractors, staff members of the unemployment insurance system, or individual researchers. Manuscripts and comments from interested individuals are welcomed. All correspondence should be sent to:

UI Occasional Paper Series
UIS, ETA, Department of Labor
200 Constitution Ave, N.W. Room S4519
Washington, D.C. 20210

Arrangements have been made for the sale of most of the reports in the series through a Federal information and retrieval system, the National Technical Information Service (NTIS). Copies of the reports are available from NTIS in paper or microfiche. The NTIS accession number and the price for the paper copy are listed after the title of each paper. The price for a microfiche copy of a paper is $4.50. To obtain the papers from NTIS, the remittance must accompany the order and be made payable to:

National Technical Information Service
U.S. Department of Commerce
5285 Port Royal Road
Springfield, Virginia 22161
Telephone: (703) 557-4650

Papers which are not available are indicated with an asterisk.

1977

G. Joachim Elterich and Linda Graham, 77-1
Impact of Extension of Coverage to Agricultural Workers Under P.L. 94-566, Their Characteristics and Economic Welfare,
University of Deleware.
NTIS PB83-147819. Price: $11.50

G. Joachim Elterich and Linda Graham, 77-1
Impact of P.L. 94-566 on Agricultural Employers and Unemployment Insurance Trust Funds in Selected States,
University of Deleware.
NTIS PB83-147827. Price: $8.50

*David Stevens, *Unemployment Insurance Beneficiary Job Search Behavior: What Is Known and What Should Be Known for Administrative Planning Purposes*, University of Missouri. 77-3

*Michael Klausner, *Unemployment Insurance and the Work Disincentive Effect: An Examination of Recent Research*, Unemployment Insurance Service. 77-4

*Gary Solon, *Weekly Benefit Amounts and Normal Weekly Wages of Unemployment Insurance Claimants*, Unemployment Insurance Service. 77-5

*Ruth Entes, *Family Support and Expenditures Survey of Unemployment Insurance Claimants in New York State, September 1972-February 1974*, New York State Department of Labor. 77-6

*Saul Blaustein and Paul Mackin, *Development of the Weekly Benefit Amount in Unemployment Insurance*, Upjohn Institute. 77-7

*Saul Blaustein and Paul Mackin, *Job Loss, Family Living Standards, and the Adequacy of Weekly Unemployment Benefits*, Upjohn Institute 77-8

1978

Henry Felder and Richard West, *The Federal Supplemental Benefits Program: National Experience and the Impact of P.L. 95-19*, SRI International. 78-1
NTIS PB83-149633. Price: $11.50.

Paul Burgess, Jerry Kingston and Chris Walters, *The Adequacy of Unemployment Insurance Benefits: An Analysis of Weekly Benefits Relative to Preunemployment Expenditure Levels*, Arizona Department of Economic Security and Arizona State University. 78-2
NTIS PB83-148528. Price: $17.50.

Christopher Pleatsikas, Lawrence Bailis and Judith Dernburg, *A Study of Measures of Substantial Attachment to the Labor Force*, Volumes I and II, Urban Systems Research and Engineering,Inc. 78-3
Vol I: NTIS PB83-147561. Price $13.00
Vol. II: NTIS PB83-147579. Price: $14.50

Henry Felder and Randall Pozdena, *The Federal Supplemental Benefits Program: Impact of P.L. 95-19 on Individual Recipients,* SRI International. 78-4
NTIS PB83-149179. Price: $13.00

*Peter Kauffman, Margaret Kauffman, Michael Werner and Christine Jennison, *An Analysis of Some of the Effects of Increasing the Duration of Regular Unemployment Insurance Benefits*, Management Engineers, Inc. 78-5

Jerry Kingston, Paul Burgess and Chris Walters, *The Adequacy of Unemployment Insurance Benefits: An Analysis of Adjustments Undertaken Through Thirteen and Twenty-Five Weeks of Unemployment*, Arizona Department of Economic Security and Arizona State University. 78-6
NTIS PB83-149823. Price: $19.00

Walter Nicholson and Walter Corson, *The Effect of State Laws and Economic Factors on Exhaustion Rates for Regular Unemployment Insurance Benefits: A Statistical Model*, Mathematica Policy Research. 78-7
NTIS PB83-149468. Price $14.50

Louis Benenson, *Incidence of Federal Retirees Drawing UCFE Benefits, 1974-75,* Unemployment Insurance Service. 78-8
NTIS PB83-161927. Price: $7.00

1979

Henry Felder, *A Statistical Evaluation of the Impact of Disqualification Provisions of State Unemployment Insurance Laws*. SRI International. 79-1
NTIS PB83-152272. Price: $17.50

Arthur Denzau, Ronald Oaxaca and Carol Taylor, *The Impact of Unemployment Insurance Benefits on Local Economies--Tucson*, University of Arizona. 79-2
NTIS PB83-169912. Price: $11.50

Paul Burgess, Jerry Kingston and the Research and Reports Section of the Unemployment Insurance Bureau, Arizona Department of Economic Security, *Labor Market Experiences of Unemployment Insurance Exhaustees,* Arizona Department of Economic Security and Arizona State University. 79-3
NTIS PB83-224162. Price: $22.00

Carolyn Sperber, An Evaluation of Current and Alternative Methods of Determining Exhaustion Ratios, Unemployment Insurance Service. 79-4
NTIS PB83-148866. Price: $8.50

Mamoru Ishikawa, Unemployment Compensation in Varying Phases of Joblessness, Unemployment Insurance Service. 79-5
NTIS PB83-150581. Price: $8.50

Nicholas Kiefer and George Neumann, The Effect of Alternative Partial Benefit Formulas on Beneficiary Part-Time Work Behavior, National Opinion Research Center. 79-6
NTIS PB83-146811. Price: $11.50

1980

Mamoru Iskikawa, Unemployment Insurance and Proliferation of Other Income Protection Programs for Experienced Workers, Unemployment Insurance Service. 80-1
NTIS PB83-140657. Price: $10.00

UI Research Exchange. Information on unemployment insurance research. First issue: 1980, Unemployment Insurance Service. 80-2
NTIS PB83-148411. Price: $17.50.

Raymond P.F. Fishe and G.S. Maddala, Effect of Unemployment Insurance on Duration of Unemployment: A Study Based on CWBH Data for Florida, Florida State University and University of Florida. 80-3
PB88-162464. Price: $19.95

*Jerry Kingston, Paul Burgess, Robert St. Louis and Joseph Sloane, Benefit Adequacy and UI Program Costs: Simulations with Alternative Weekly Benefit Formulas, Arizona Department of Economic Security and Arizona State University. 80-4

1981

UI Research Exchange. Information on unemployment insurance research. First issue: 1981. Unemployment Insurance Service. 81-1
NTIS PB83-152587. Price: $19.00

Jerry Kingston, Paul Burgess, Robert St. Louis and Joseph Sloane, Can Benefit Adequacy Be Predicted on the Basis of UI Claims and CWBH Data? Arizona Department of Economic Security and Arizona State University. 81-2
NTIS PB83-140566. Price: $8.50

Paul Burgess, Jerry Kingston, Robert St. Louis and Joseph Sloane, Changes in Spending Patterns Following Unemployment, Arizona Department of Economic Security and Arizona State University. 81-3
NTIS PB83-148833. Price: $8.50

UI Research Exchange. Information on unemployment insurance research. Second issue: 1981, Unemployment Insurance Service. 81-4
NTIS PB83-148429. Price: $14.50

1983

Walter Corson and Walter Nicholson, An Analysis of UI Recipients' Unemployment Spells, Mathematica Policy Research. 83-1
NTIS PB84-151463. Price: $14.50

Lois Blanchard and Walter Corson, A Guide to the Analysis of UI Recipients' Unemployment Spells Using a Supplemented CWBH Data Set, Mathematica Policy Research. 83-2
NTIS PB84-151471. Price: $16.00

Ronald L. Oaxaca and Carol A. Taylor, The Effects of Aggregate Unemployment Insurance Benefits in the U.S. on the Operation of a Local Economy, University of Arizona. 83-3
NTIS PB84-150317. Price: $10.00

UI Research Exchange. Information on unemployment insurance research. 1983 issue. Unemployment Insurance Service. 83-4
NTIS PB84-150325. Price: $14.50

1984

UI Research Exchange. Information on unemployment insurance research. 1984 issue. Unemployment Insurance Service. 84-1
NTIS PB85-180370. Price: $17.50

Stephen Wandner, John Robinson and Helen Manheimer. 84-2
*Unemployment Insurance Schemes in Developing
Countries*, Unemployment Insurance Service.
NTIS PB85-185098/AS. Price: $11.50

1985

Walter Corson and Walter Nicholson, *An Analysis of* 85-1
the 1981-82 Changes in the Extended Benefit Program,
Mathematica Policy Research.
NTIS PB85-176287/AS. Price: $13.00

Walter Corson, David Long and Walter Nicholson, 85-2
*Evaluation of the Charleston Claimant Placement and
Work Test Demonstration*, Mathematica Policy Research.
NTIS PB85-152965. Price: $14.50

Walter Corson, Alan Hershey, Stuart Kerachsky, 85-3
Paul Rynders and John Wichita, *Application of
the Unemployment Insurance System Work Test and
Nonmonetary Eligibility Standards*, Mathematica Policy
Research.
NTIS PB85-169910/AS. Price: $17.50

Robert Moffitt, *The Effect of the Duration of* 85-4
*Unemployment Benefits on Work Incentives: An
Analysis of Four Data Sets*, Mathematica Policy
Research.
NTIS PB85-170546. Price: $14.50

Helen Manheimer and Evangeline Cooper, *Beginning* 85-5
the Unemployment Insurance Program--An Oral History,
Unemployment Insurance Service.
NTIS PB87-117370/AS. Price: $16.95

1986

Helen Manheimer, John Robinson, Norman Harvey, 86-1
William Sheehan and Burman Skrable, *Alternative
Uses of Unemployment Insurance*, Unemployment
Insurance Service.
NTIS PB87-118402/AS. Price: $16.95

Norman Harvey, *Unemployment Insurance Bibliography*, 86-2
Unemployment Insurance Service.
NTIS PB87-118410/AS. Price: $21.95

Walter Corson, Jean Grossman and Walter Nicholson, 86-3
*An Evaluation of the Federal Supplemental
Compensation Program*, Mathematica Policy Research.
NTIS PB86-163144. Price: $16.95

Stuart Kerachsky, Walter Nicholson and Alan Hershey, 86-4
An Evaluation of Short-Time Compensation Programs,
Mathematica Policy Research.
NTIS PB86-167616. Price: $22.95

James M. Rosbrow, Fifty Years of Unemployment Insurance--A Legislative History: 1935-1985, Unemployment Insurance Service. 86-5
NTIS PB87-179834/AS. Price: $18.95

Stephen A. Wandner, (editor) Measuring Structural Unemployment, Unemployment Insurance Service. 86-6
NTIS PB87-209433/AS. Price: $18.95

1987

Burt Barnow and Wayne Vroman, An Analysis of UI Trust Fund Adequacy, Unemployment Insurance Service. 87-1
NTIS PB67-209342. Price: $6.95

Esther Johnson, Short-Time Compensation: A Handbook Basic Source Material, Unemployment Insurance Service 87-2
NTIS PB88-163589 Price: $19.95

1988

Walter Corson, Stuart Kerachsky and Ellen Eliason Kisker, Work Search Among Unemployment Insurance Claimants: An Investigation of Some Effects of State Rules and Enforcement. Mathematica Policy Research. 88-1
NTIS PB89-160022/AS. Price: $28.95

UI Research Exchange. Information on unemployment insurance research. 1988 issue. Unemployment Insurance Service. 88-2
NTIS PB89-160030/AS. Price: $21.95

Walter Corson and Walter Nicholson, An Examination of Declining UI Claims During the 1980s. Mathematica Policy Research. 88-3
NTIS PB89-160048/AS. Price: $21.95

Phillip Richardson, Albert Irion, Arlen Rosenthal and Harold Kuptzin, Referral of Long-Term Unemployment Insurance (UI) Claimants to Reemployment Services. Macro Systems and Mathematica Policy Research. 88-4
NTIS PB89-153100/AS. Price $28.95

1989

Walter Corson, Walter Nicholson and Stuart Kerachsky, The Secretary's Seminars on Unemployment Insurance. Mathematica Policy Research. 89-1
Available soon at NTIS

Phillip Richardson, Albert Irion, Arlen Rosenthal and Harold Kuptzin, Referral of Long-Term Unemployment Insurance (UI) Claimants to Reemployment Services. Second Edition. Systems and Mathematica Policy Research. 89-2
Available soon at NTIS

Walter Corson, Shari Dunstan, Paul Decker, and Anne Gordon, New Jersey Unemployment Insurance Reemployment Demonstration Project. Mathematic Policy Research. 89-3
Available Soon at NTIS

☆ U.S. GOVERNMENT PRINTING OFFICE 1989—241-410 06547